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THE SCIENTIFIC MONTHLY

FEBRUARY, 1921

HISTORY OF GEOLOGY*

By Professor HERBERT E. GREGORY

YALE UNIVERSITY

INTRODUCTION

THE history of geology is essentially the history of the intelligent observation of rocks, fossils and land forms. Progress is marked by progressive increase in exactness and completeness of observation. In an atmosphere saturated with tradition and personal bias the making of observations and the interpretation of observations present but a sickly growth; and when the intellectual environment includes authority and a complete outfit of supernatural causes, growth is stopped entirely. This may in part account for the interesting fact that philosophy and literature rather than observational science represent the intellectual efforts of the ancients. Poetry and musings about the nature of things require no special technique, no collections of materials for comparison. A gifted mind is the essential equipment; and such minds may appear anywhere and at any time. But the development of natural science involves critical observation of a variety of things from many places, the interchange of ideas among many workers, the making of hypotheses, the formulation and selection of method, and the invention of apparatus. It is natural that this group of requirements should come together slowly.

In no real sense can the obvious geological truths irregularly interwoven in the interesting fabric of myth and fact which constitutes Greek, Arabie, Indian, and Chinese thoughts on Nature be considered the beginnings of geology. The traditions of the Mediterranean peoples, of the Hebrews, Babylonians, and Hindus, are rich in speculation and in the making of hypotheses regarding earth origin, but poor in logical deduction from exact observation. They show little interest in the earth itself and no inkling that the history of the earth is to be deciphered by means of fossils, knowledge of the earth's crust, and the action of rivers and waves. The test is the extent to which

*Lecture delivered at Yale University, November 18, 1920, in a series on the History of Science under the auspices of the Yale Chapter of the Gamma Alpha Graduate Scientific Fraternity.

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the contributions of the ancients were utilized as stepping stones. In geology progress has been attained without regard to or even in ignorance of observation and theories recorded before the late Middle Ages. Cuvier established paleontology without reference to the teachings of previous times and even in ignorance of the work of his contemporaries, and Darwin acknowledged his indebtedness to Lamarck, not to Aristotle, whose theory of evolution lacked little of being complete. Likewise structural geology, stratigraphy, and physiography have grown up without assistance from classical and Middle Age scholars. The fifteenth century student of earth science enjoyed a surprisingly meager heritage from classical and early Christian days. In the sixteenth century Leonardo da Vinci stands alone. During the seventeenth century many sciences made great strides forward; new facts were unearthed and methods established. Physics received the contributions of Galileo, Kepler, Newton, Torricelli, Guericke, Boyle, Huygens, Hooke. Astronomy, already far advanced, was revolutionized by the development of the telescope, and biology by the microscope. Chemistry found a place apart from alchemy and medicine. In geology, on the other hand, the seventeenth century scholar was groping in darkness scarcely less dense than that surrounding his predecessors of the sixteenth and fifteenth centuries.

Toward the close of the eighteenth century many of the facts and principles and methods which constitute geology were assembled but geology as taught today is essentially a nineteenth century product to which many of the most significant contributions have been made by scholars of the present generation.

The subject matter of geology is so varied and the introduction of new views so irregularly placed in time, that no chronologic sequence appears in the growth of the science as a whole. Development may best be shown by tracing the growth of certain fundamental ideas: the origin of the earth; the meaning of rocks, mountains, surface features and fossils; and the geologic time scale.

Origin of the Earth

From direct observation geology knows nothing of the original earth; no part of its first formed surface has been seen or is likely ever to be seen. The oldest rocks known have doubtless been derived from rocks yet older, and the oldest fossil undoubtedly descended from a long line of still older organisms. The evolution of the physical earth and of life clearly point to a period charged with dynamic and vital forces long antedating the most ancient legible records.

It must be admitted that so far little clear light has been thrown on the origin and primeval condition of the earth. For the geologist

there is little choice between the childlike myths of the Eskimos, Bushmen, and Micronesians, the grand poetic conceptions of the Hindus, Babylonians, and Hebrews, and the pseudoscientific teachings of the Greeks and medieval churchmen.

An advance is recorded during the seventeenth century in the contributions of Descartes (1596-1650) and Leibnitz (1646-1716) who traced the development of the earth from a disordered mass of glowing material to a smooth, solid globe, the exterior of which had cooled. These ideas were expanded and built into a consistent theory through the labors of Kant (1755) and especially of Laplace (1796)[1], whose views of earth origin received well-nigh universal acceptance during the nineteenth century. In essence the nebular hypothesis of Laplace assumes the existence of highly heated gaseous nebulae slowly rotating about a central mass which eventually became the sun. As this nebulous material rotated, cooled, and contracted, rings of matter were detached one after another furnishing the stuff for planets and satellites. One of these rings, gathered into a spheroid, became the earth. The original earth, therefore, was a luminous star surrounded with a heavy vaporous atmosphere. The ball passed from a gaseous to a liquid state and developed a wrinkled crust of igneous rock like granite. Later the atmosphere gave rise to oceans and streams, agents for the production of sedimentary rock.

The nebular hypothesis is the crowning achievement in cosmical geology up to the end of the nineteenth century, but its value lies not so much in its inherent probability as in the absence of a better theory. It violates the principles of thermo-dynamics and of celestial mechanics and is out of accord with the present knowledge of nebulae, planets and satellites. Furthermore, the theory demands progressive cooling of the earth, and an arrangement of rock masses amply disproved by geological evidence. Without radical reconstruction, the nebular hypothesis can no longer serve as a reasonable theory of earth origin.

The underlying conceptions of the nebular hypothesis are: first, the condensation of diffuse matter under the action of gravity, and second, nebulae distended by heat and revolving as a unit mass. But the researches in astronomy and physics during the past quarter century have accumulated evidence to show that disruption and repulsion, not attraction, are the dominant forces in the stellar universe. The tails of comets turned away from the sun, streamers thrown from the sun itself, and the shape of certain star clusters and nebulae point to prodigious repelling forces within the luminous bodies making up the universe. On such evidence the planetesimal hypothesis of Chamberlin and Moulton is founded.[2] Under this theory the earth was once

[1]Laplace, P. S., Exposition du système du monde, Paris, 1796 and 1824.
[2]Chamberlin, T. C., The origin of the earth, University of Chicago Press, 1916.

a spiral nebula composed of matter "thrown out" by some ancestral sun. The scattered particles, or planetesimals, of the parent nebula were drawn into nuclei which became part of the planetary system. At this stage the cosmic history of the earth passes into the geological history. The original earth is conceived as a ball 2,000 to 3,000 miles in diameter, which grew to its present size by the addition of more planetesimals. The heat of the earth comes from self-condensation and progressive close-packing of its constituent planetesimals. Under this theory vulcanism was active long before the earth attained its present size, and an atmosphere appeared as soon as the earth possessed sufficient gravitative power to retain it. When the atmosphere became saturated with aqueous vapor, water was formed and occupied depressions in the earth's uneven surface.

The truth of the planetesimal hypothesis remains to be separated from its errors by a long period of testing and developing. Its value lies in the fact that it explains a great number of geological observations and suggests lines for future investigation.

Meaning of Rocks

The origin of the earth and the history of life on this planet are involved in religious and philosophic views and therefore precede in point of time the study of the materials of which the earth is composed. At the beginning of the nineteenth century some progress had been made in the knowledge of minerals[3] but so little was known of the composition and texture of rocks that masses of igneous origin were confused with strata laid down by water or by wind, and the existence of vast exposures of metamorphic rocks was not recognized. The distinction between a rock and a geological formation or group of strata had not been fully established, and many fine-grained rocks were classed as minerals. As late as 1837 the Munich chemist, Johann Fuchs, contended strenuously for the view that mica schist, granite, and porphyry were the results of the consolidation of a watery paste. Only within the past fifty years has the systematic investigation of rocks—their composition, relations and origins—reached a stage that justified the recognition of a distinct branch of geologic science. Since the importance of its contributions has been demonstrated, the study of rocks has experienced two somewhat distinct but logical periods of development. *Petrography*, the description of rocks, is a necessary forerunner to *Petrology*, researches in the origin and broader relations of rocks.[4]

[3]Ford, W. E., The growth of mineralogy from 1818 to 1918: A century of science in America, pp. 268-283, Yale University Press, 1918.

[4]Pirsson, L. V., The rise of petrology as a science: A century of science in America, pp. 248-267, Yale University Press, 1918.

Cross, Whitman, The development of systematic petrography in the nineteenth century: *Jour. Geology*, X, 332-376, 451-499, 1902.

The stages of advancement in petrography may be traced by noting the systems of classifications in vogue at different periods, for classification involves the application of all known facts about all known kinds of rocks and also a consideration of existing theories and assumptions. The classifications of rocks based on hardness, specific gravity, and geographical location, are obviously superficial and one may dismiss as a humorous but futile notion the dictum of Jameson that there is but one species in mineralogy, namely, the globe, and the wordy argument of Pinkerton (1811) that no *species* of minerals exist for no mineral has the capacity to reproduce its kind. It is easy to understand, however, that students of rocks should have placed different emphasis on chemical composition, texture, mineralogical composition, age, mode of occurrence, and origin, as criteria, and should be of different minds regarding the desirability of a "natural classification" as opposed to an artificial one.

Early in the eighteenth century contributions to the knowledge of rocks were made by the few men who resisted the temptation to speculate and to dogmatize about things in general, and who confined their attention to a particular topic or a particular locality. In 1768 Linnaeus (1707-1778) extended his *Systema Naturae* to include the inorganic kingdom which he divided into rocks, minerals, and fossils. To each of these subdivisions was assigned an incongruous group of materials. As remarked by Cross, the most visible effect of this pioneer attempt to force inorganic substances into the scheme of species and genera provided for plants and animals was to furnish a theme for controversial debates and arguments for a century to come.

The outstanding figure among students of rocks of the eighteenth and early nineteenth centuries is Abraham Gottlob Werner (1749-1817), professor of mineralogy at Freiberg, whose enthusiasm, eloquence, skill in teaching, and clear methodical presentation attracted learners from all parts of Europe. Werner's views of rock genesis and of geological processes were antiquated even for his time, but his painstaking systematic examination of rocks led to a classification based on mineral composition (1786)—a feature common to modern schemes. He distinguished "simple" from "compound" rocks, recognized that some minerals were "essential" components of rocks and others "incidental" or "accessory," and clarified the subject by drawing the distinction between rock masses or strata (formations) and the rocks composing them, thus laying the foundation of modern descriptive petrography. During the first two decades of the nineteenth century, the knowledge of rocks as summarized in systems of classifications was carried as far as possible under the Wernerian scheme by Haüy (1801), Brongniart (1813), Cordier (1815), and their contemporaries who relied upon mineralogical composition and structure to indicate rela-

tionship to the exclusion of age, origin, and mode of occurrence.

But even during this period the problem of rock origin was prominently in mind. Were all rocks deposited by the ocean as chemical precipitates, as taught by Werner, or do deep-seated igneous rocks and lavas and sedimentary rocks indicate three modes of origin as taught by Hutton? Do the different kinds of rocks represent merely different ages of accumulation or are granite and sandstone made in all ages, even today? Are gneiss and schist original igneous rocks or altered sedimentary rocks? As if by common consent, the origin of the lava, basalt, was taken as a test case, and geologists, chemists, physicists, and even literary men and politicians divided into two camps—the Neptunists who contended that basalt is deposited from sea water were vigorously opposed by the Plutonists who believed in an igneous origin. Peace was declared in favor of the Plutonists soon after it was agreed that field observations were better weapons than arguments concocted in the library.

A distinct advance in the knowledge of rocks is recorded by two publications in the third decade of the nineteenth century.[5] Von Leonhard's "Characteristics of Rocks" (1823) is listed by Cross as "the first fairly consistent treatise on rocks" and its author as "unquestionably the foremost petrographer of his day, sharing with Alexander Brongniart the honor of placing the classification of rocks on a firm basis as a systematic science." Through the work of these able minds the confusion heretofore existing between minerals, rocks, and assemblages or groups of rocks (terranes and formations) was eliminated; the study of rocks as rocks (petrography and petrology) was shown to be a branch of learning with methods and purposes different from the study of strata and masses composed of rocks (stratigraphy); and the biological scheme of genera and species was discarded as inapplicable. These workers showed that structure, as well as mineralogical composition, is a significant feature and Brongniart suggests that geological origin may have value as a principle of classification. Both authors state with refreshing candor that fuller knowledge will show that many rocks have been given inappropriate places in the scheme of classification. To his major divisions, (1) heterogeneous rocks, (2) homogeneous rocks, (3) fragmental rocks, (4) loose rocks, Von Leonhard added a group, "rocks apparently homogeneous," to care for serpentine, pitchstone, and certain schists whose constituents were not visible to the unaided eye but which were not minerals. Brongniart subdivided "homogeneous rocks" into those with distinct known mineral species and those whose density precluded the recognition of constituents.

[5]Von Leonhard, K. C., Charakteristik der Felsarten, 1823.
Brongniart, Alexandre, Classification et caractères minéralogiques des roches homogènes et hétérogènes, Paris, 1827.

A new mode of treatment was introduced into the science of rocks by Carl Friedrich Naumann (1850 and 1858)[6]. Under the name *Petrography* he defined the scope of the science of rocks as a branch of Geology (or *Geognosie* as the term was then used) which could be studied from six standpoints: the constituents of rocks, the texture and structure of rocks, manner of occurrence, systematic description, genesis of rocks, and alteration of rocks. He introduced the classification: (1) crystalline rocks, (2) clastic rocks, (3) rocks neither crystalline nor clastic.

Von Cotta's contribution[7] (1855 and 1862) was the emphasis placed on geological mode of origin and the clear expression of the modern view that molten material poured from volcanoes and molten material formed deep within the crust of the earth may be crystallized into rock during any geological epoch and are not therefore indicative of age.

Frederick Senft (1857), probably impressed by the difficulty of determining the characteristics of dense rocks, minimized the value of mineralogical composition, texture, and structure as interpretative guides and developed an elaborate and highly artificial scheme based on chemical composition. But the master mind of the group whose attention was directed to the chemical relationship of rocks was Justus Roth. From a careful study of nearly 1,000 analyses he reached the conclusion (1861) that rocks cannot be represented by chemical formulae which coincide with mineralogical composition, and that the application of the chemical factor as a criterion in classification serves to separate rocks otherwise closely related. As a substitute he proposed that igneous rocks be grouped with reference to the abundance and kind of feldspar crystals contained within them.

An opposite conclusion was reached by Sheerer (1864) who expressed the belief that igneous rocks could be satisfactorily grouped in nine chemical types.

Zirkel's *Lehrbuch der Petrographie* (1866) and the philosophical discussions of Von Richthofen (1868) are substantially restatements of earlier views but are worthy of study as expressions of the usages and beliefs of the time and as the culmination of efforts to describe and to interpret rocks on the basis of superficial characteristics and approximate chemical analyses..

By 1870 the possibilities of increase of knowledge through the study of rocks appeared to have been exhausted; no further steps of advance seemed possible, for the components of fine-grained rocks, lavas, and schists were beyond the reach of observation and there ap-

[6]Naumann, C. F., Lehrbuch der Geognosie, 1850.

[7]Cotta, Bernhardt von, Rocks classified and described; a treatise on lithology (trans. P. H. Lawrence, 1866).

peared to be no satisfactory means of distinguishing the varieties of feldspars, the most abundant ingredient in the commonest rocks. Petrography had come to a blank wall. Further research involved the discovery of some method for more complete and exact observation. The need was met by the introduction of the compound polarizing microscope which brings to view and differentiates minerals even in apparently homogeneous rocks. The development of this instrument and of the means of preparing rocks for study marked the beginning of the golden age of descriptive petrography, the last quarter of the nineteenth century. The way had been blazed by Professor Nicol, the Scotch geologist, who invented the Nicol prism for polarizing light, attached it to a microscope, and devised a method for preparing thin sections of fossil wood (1828). The success of this method led Ehrenberg to the epoch-making discovery that chalk and marls and some limestones were composed of skeletons of organisms. Sorby (1850) used this method for determining the composition of sandstone and discussed its value for the study of igneous rocks. But to make a chip of hard rock sufficiently thin to be transparent seemed a hopeless task. It is a triumph of technical skill to cut from a black dense rock a section 1/1000 of an inch thick through which print may be read and which reveals to the microscope the minutest structures. The seemingly impossible has been accomplished and the modern geologist is placed in the position of the biologist with respect to the examination of microscopic objects of natural history. This method of research under the lead of Zirkel and Rosenbusch in Germany, Michel-Lévy, Barrois, and Lacroix in France, Bonney, Judd, and Rutley in England, E. S. Dana, G. H. Williams, and Iddings in America, promised so much that it soon enlisted an army of workers who added enormously to the knowledge of rocks and of the minerals composing them. During the closing years of the nineteenth century, microscopic description of rocks appeared to be the chief aim of petrographers.

About the beginning of the present century *Petrography* became *Petrology;* the science of the exhaustive description of rocks became the science of relations and meaning of rocks. The genesis of rocks and the factors that have brought about their geographic distribution and produced the hundreds of varieties are topics of interest to a modern student of petrology. The goal is in sight, but the best means of reaching the goal is not apparent. As in other lines of research, progress depends upon choice of method. Reliance on the petrographic microscope has revealed a new world to geologists, but it has obvious limitations. It is an instrument for collecting data, for refined and accurate description rather than for determining origins, and after all known rocks and rock-making minerals have been studied this method has served its main purpose. This stage nearly has been

reached. Twenty years ago most minerals, certainly all those of wide distribution, had been exhaustively studied and igneous rocks by the thousands had been minutely described and built into schemes of classification. Many sedimentary rocks and schists and gneisses also have been added to the list. Progress has been attained by the development of chemical methods of research in rock origins and rock-relationship. The pioneer work of Bischof (1846), the founder of chemical geology, Bunsen (1851) and Senft (1857), led the way to the researches of Roth, Clarke, and Hillebrand, and culminated in Washington's awe-inspiring volume "Chemical Analyses of Igneous Rocks" (1903), Cross, Iddings, Pirsson, and Washington's "Quantitative Classification of Igneous Rocks" (1903), and Clarke's "Data of Geochemistry" (4th ed. 1920)—three American works which are essential handbooks for geologists and chemists of all countries.

But while it is generally admitted that chemical composition is the most fundamental characteristic of rocks, it is obvious that the most precise determination of the chemical constituents of all the rocks in existence would not in itself explain the origin of rocks or contribute more than unrelated facts to the history of the earth. In order to gain the truths of rock history it is necessary to know the processes which cause the results and the conditions under which these processes operate. On the basis of chemical composition, by theoretical and to a small extent by experimental methods, interesting attempts were made during the last quarter of the nineteenth century to determine the order in which minerals crystallize from a molten mass (magma) and the conditions responsible for the differentiation of magmas into chemical groups. As at other stages in the history of petrology, the problem was recognized but the known methods were inadequate.

The gateway to further research was opened by physical chemistry. With the development of this new science and the consequent improvement in experimental methods came the possibility of reproducing in the laboratory the work of underground forces and of recording the stages through which rocks and minerals pass from undifferentiated masses of molten or liquid material to their final form as quartz, granite, or marble.

In view of modern developments it is interesting to recall pioneer experiments. To disprove the teaching of the all powerful German School of his day that basalt (lava) was precipitated from water, Sir James Hall in the year 1800 melted lavas from Etna and Vesuvius and allowed the mass to cool. Solid crystalline rock material resulted. Daubrée (1857) made quartz and feldspar from an aqua-igneous complex, proving that the conditions necessary to produce "granite-grained" igneous rock were moderate temperatures and presence of water vapor. Fouque and Michel-Lévy (1878) produced augite-

andesite with well-developed crystals by fusing selected ingredients in a dry state holding the fused mass at a high temperature for forty-eight hours then allowing it to cool. These brilliant researches of French geologists were carried still farther by Vogt and by other European scholars. But the world center for the experimental study of rock genesis is the Geophysical Laboratory at Washington[8] where under ideal conditions a corps of physicists, chemists, mineralogists, and petrologists are solving the deeper problems of rock genesis and rock relationship.

The Making of Mountains

Since the dawn of human history, even the uncritical observer must have noted that rock masses differ not only in color and composition but also in attitude; that some strata lie flat, others are tilted, still others are folded and buckled or broken. On the theory of a ready-made earth such facts occasioned no comment but as the evidence accumulated that changes large and small have affected the earth's surface, speculations regarding the causes and processes of rock disturbance and of the origin of mountains were in order.

To observers of the seventeenth century earthquakes were an all sufficient cause. Hooke (1688)[9] expressed the belief:

> Earthquakes have turned plains into mountains and mountains into plains, seas into land and land into seas, made rivers where there were none before, and swallowed up others that formerly were.

Woodward (1695)[10] cut the knot with the statement,

> the whole terrestrial globe has been taken to pieces at the flood and the strata settled down from this promiscuous mass.

Burnet[11] took the same view, and the state of knowledge of the times may be judged from the fact that his theory of the earth (1690) thoroughly unsound in matter, method and conclusions was praised in a Latin ode by Addison and highly commended by Steele.

During the eighteenth century the view prevailed that all rocks were originally horizontal and that departures from this attitude were local and sufficiently accounted for by landslides, by cavities into which rocks fell, by volcanoes, and by original deposition in addition to the ever ready earthquake or flood which played the title rôle. As late as 1823 the easterly dip of the Connecticut River sandstone is ascribed by Hitchcock to "some Plutonian convulsion."[12]

[8]Carnegie Geophysical Laboratory, Washington. For a sketch of the scope and contributions of this institution, see Sosman, R. B., The work of the geophysical laboratory of the Carnegie Institution of Washington: A century of science in America, pp. 284-287, Yale University Press, 1918.

[9]Hooke, Robert, Posthumous works, ed. R. Waller, London, 1705.

[10]Woodward, John, Essay towards a natural history of the earth, 1695.

[11]Burnet, Thomas, Telluris theoria sacra, or Sacred theory of the earth, London, 1681. Eng. trans. 1684-1689.

[12]Hitchcock, Edward, Geology, etc., of the Connecticut Valley; *Am. Jour. Sci.*, VI, 74, 1823.

Toward the close of the eighteenth century belief in the ability of streams and waves to corrade the surface, and to carry debris into the ocean, gained general acceptance. This belief carried to its logical conclusion meant that all dry land would ultimately disappear unless some forces were acting to re-elevate the continents. Earthquakes might break strata and volcanoes scatter the material about, but their effect is local and it was difficult to imagine how they might raise and depress the sea floor, build high mountains, or even produce the folds and contortions characteristic of many regions. Even the advocates of the Noachian flood were forced to depart from the literal description and call in comets and sudden shifting of the earth's axis to account for the seemingly disorganized earth with marine shells miles high on mountains. It was seen that some new mechanism must be devised, but the accepted teachings of the eighteenth and early nineteenth centuries allowed no place for an additional agent. Out of this impasse geology was led by James Hutton—successful physician, farmer, and manufacturing chemist. Discarding speculation and tradition and all concern for origins of things, this "patient, enthusiastic, level-headed devotee of science" observed phenomena and processes, and developed a logical theory which lies at the base of modern dynamical geology. Hutton's "Theory of the Earth with Proofs and Illustrations" (1795) and its companion volume, Playfair's "Illustrations of the Huttonian Theory" (1802), are classics in geologic literature, which are scarcely out of place in a modern class room. The scheme as outlined by Hutton is simple and convincing. Observation taught him that the features of the earth are not rigid and immutable but are continuously undergoing changes. Rocks decay, soil is swept away by streams, coasts are worn down, and all loose material is carried to the sea. In time the solid lands must disappear. The debris is deposited on the ocean floor forming layers in which remains of organisms are embedded. The material for making future lands is thus prepared. But to be recovered from the sea and built into continents these sediments must be elevated. In searching for an agent capable of causing uplift, Hutton dismissed as phantoms the "convulsions of nature," "emanations," and "universal debacles" of his contemporaries and predecessors. Going once more to the field, he observed that many rocks are not stratified and many are bare of fossils and that these rocks show unmistakable evidence that they were once in a molten state; in fact, that some of the igneous materials have come up from below, penetrated the surrounding rocks, and altered their appearance and composition. Deep within the earth, therefore, heat must prevail and the sudden expansion of rocks induced by heat not only produced volcanoes but lifted the overlying rock masses. Rugged mountains, broken and tilted strata, and folds are witnesses to these gigantic upheavals.

Hutton's teachings were a half century ahead of his time and made slow headway. Though supported by the nebular hypothesis of a cooling globe, by the testimony of miners that heat increases with depth, and by the evidence of volcanoes as presented by Desmarest (1725-1815) and by Scrope (1823), the hypothesis of universal and subterranean heat was ignored or combatted by the strongly entrenched Wernerian school which clung to the view that all rocks are formed from water, that mountains are gigantic crystalline aggregates made where they stand, and that the earth is cold to the center. Professor Jameson, a colleague of Playfair at Edinburgh University, writing in 1808[13], calls the researches of Hutton, Playfair and Hall "monstrosities" and remarks: "It is therefore a fact that all inclined strata with few exceptions have been formed so originally and do not owe their inclination to subsequent change." Fortunately for science Hutton was followed by Lyell (1797-1875). Taking for his text the saying of Hutton, "Amid all the revolutions of the globe, the economy of nature has been uniform," Lyell expounded and systematized the theories of his master, gathered new facts, pointed out errors, and through his "Principles of Geology" guided the thought of students during the second quarter of the nineteenth century. Lyell's chief contribution was the development of the thesis that the forces operating on and within the earth during past time are the same as those of today; that knowledge of past events is to be gained by studying present processes. The building of mountains and continents, the folding and breaking of strata, the making of igneous and sedimentary rocks, and the entombment of fossils are proceeding as rapidly and in the same manner as in other ages. There have been no "gigantic cataclysms" or "devastating floods"; all processes have been orderly and uniform in degree and in kind. The emergence and submergence of coasts, the changes of level associated with earthquakes, are the rule not exceptions, and do not involve unusual forces.

> The land has never in a single instance gone down suddenly for several hundred feet at once. . . . Great but slow oscillations have brought dry land several thousand feet below sea and raised it thousands of feet above. Places now motionless have been in motion and places of present active movements were formerly stationary.

Although this doctrine of "uniformitarianism" was carried by Lyell somewhat beyond the modern viewpoint the road to progress was cleared of fantastic speculations.

Hutton and Lyell considered heat combined with pressure sufficient cause for vertical uplifts of parts of the earth's surface. The analyses of these processes have absorbed the attention of structural geologists down to the present day. In 1833 the brilliant French scholar, Elie de Beaumont (1798-1874), expressed the view that the earth is a fused

[13] Jameson, Robert, Elements of geognosy, Edinburgh, 1808.

mass covered by an envelope of cooled rock "thinner in proportion than the shell of an egg." In adjusting itself to the cooling interior this crust became wrinkled. From time to time portions of the crust collapsed along definite lines of fracture. At such times the rocks are subjected to great lateral pressure; the unyielding ones are crushed, the pliant ones bent and forced to pack themselves into smaller space. The readjustment of the shell to the shrinking interior causes portions of the crust to be squeezed upward as wrinkles or folds which we call mountain ranges. By reference to the surrounding rocks, the date of the mountain's birth is obtained.

These theoretical views although erroneous and discounted even during the life time of their author marked an important advance, for through them came the idea of mountain folding by lateral compression. As treated by James D. Dana,[14] this conception grew into a consistent theory of mountain origin and structure which has received universal acceptance. In brief, this theory is as follows: Materials for the future mountain system are eroded from a land mass and deposited in a progressively sinking trough to thickness of thousands of feet. After long ages the sediments in the trough are compressed laterally against the relatively solid old land; the shortening, amounting to many miles (Appalachians, 40 miles; Alps, 74), is made possible by folding or by forcing parts to override other parts. During and after the periods of folding and faulting the newly born mountain range is eroded into features which are recognized as ridges, peaks and valleys. These processes, which in detail are enormously complicated, involve regional upwarps and downwarps which are recorded over wide areas. Largely through a study of mountain ranges with their faults and folds and enormous thicknesses of disturbed sedimentary and igneous rocks has come the modern view of the fundamental structural relations: that the earth is not a liquid or molten mass covered with a crust, but a globe as rigid as a ball of steel or glass of equal dimensions yet "elastic" or "pliable" enough to yield under the weight of even a moderate load.

Interpretation of Natural Scenery

A discussion of the principles and processes involved in sculpturing the earth surface was futile on the hypothesis of a ready-made earth whose features were unchangeable except when modified by catastrophic action. The belief in the Deluge as the one great event in geological history effectually checked investigation of the work of rivers, glaciers, wind, and the atmosphere in producing the variety of forms that constitute natural scenery. It is therefore not surprising that physiography, whose essence lies in the belief that present land forms rep-

[14]Dana, J. D., Manual of geology, Philadelphia, 1863, 3d ed., 1880.

resent merely a stage in the orderly development of the earth's surface features, should have attained the dignity of a science within the past quarter century; nor that the speculations of Aristotle, Herodotus, Strabo, and Ovid, and the illustrious Arab, Avicenna (980-1037), unchecked by appeal to facts but also unopposed by priesthood or popular prejudice, are nearer to the truth than the intolerant controversial writings of the intellectual leaders of the late Middle Ages whose touchstone was orthodoxy. Steno (1638-1687) mildly suggested that surface sculpturing, particularly on a small scale, is largely the work of running water, and Guettard (1715-1786) grasped the fundamental principles of denudation; but nearly eighteen centuries had elapsed before Desmarest, the father of physiography, presented proofs that valleys are made by rivers and that a landscape passes through clearly defined stages of development.

Desmarest's teachings were strengthened and expanded by DeSaussure (1740-1799)[15], the originator of the term, "geology," who saw in the intimate relation of Alpine streams and valleys the evidence of erosion by running water (1786).

These works from the acknowledged leaders of geological thought of the period aroused singularly little interest on the Continent, and Lamarck's volume on denudation (Hydrogéologie), which appeared in 1802, although an important contribution, sank out of sight. But the seed of the French school found fertile ground in Edinburgh, the hub of the geological world at the close of the eighteenth century. Hutton's "Theory of the Earth, with Proofs and Illustrations," in which the guidance of DeSaussure and Desmarest is gratefully acknowledged, appeared in 1795. The original publication aroused only local interest, but when placed in attractive form by Playfair[16], the problem of the origin and development of land forms assumed a permanent position in geological thought. Steps in the analysis and solutions of these problems may be illustrated by tracing the growth of ideas regarding valleys and features produced by glaciation.

In the interpretation of valleys little progress was made during the first fifty years of the nineteenth century. Physiographic literature shows that the clear reasoning of Desmarest, DeSaussure, Hutton, and Playfair, firmly buttressed by concrete examples, was insufficient to overcome the belief that valleys are ready-made or result from cataclysms and that the corrugations and irregularities of mountain surface are remnants of the primeval earth. The principles laid down by these clear-sighted leaders were too far in advance of their time to secure general acceptance. In a paper with the significant title, "Burst-

[15]Saussure, H. H. de., Voyage dans les Alpes, 1779-1796.

[16]Playfair, John, Illustrations of the Huttonian theory, 1802; trans. into French by C. A. Bassett, 1815.

ing of Lakes Through Mountains," Wilson (1821) asks: "Is it not the best theory of the earth, that the Creator, in the beginning, at least at the general deluge, formed it with all its present grand characteristic features?"[17]

In 1823 Buckland[18] wrote:

> . . . The general belief is that existing streams, avalanches and lakes, bursting their barriers, are sufficient to account for all their phenomena. It is now very clear to almost every man, who impartially examines the facts in regard to existing valleys, that the causes now in action . . . are altogether inadequate to their production; nay, that such a supposition would involve a physical impossibility. . . . We do not believe that one-thousandth part of our present valleys were excavated by the power of existing streams.

Similar views expressed in scientific journals of Europe and of America by the leaders of geologic thought, including Hitchcock (1824),[19] Phillips (1829),[20] Lyell (1833), Conrad (1839),[21] Darwin (1844),[22] Warren (1859),[23] and Lesley (1862).[24]

By the middle of the nineteenth century opinion regarding valleys had become standardized somewhat as follows: the position of many valleys is determined by original surface inequalities or by later fractures in the earth's crust; most of them are intimately associated with earthquakes, bursting of lakes, or the sudden upheavals or depressions of the land; valleys of erosion are chiefly the work of the sea, but rivers may perform similar work on a small scale.[25] The extent of the wandering from the guidance of DeSaussure and Playfair after the lapse of fifty years is shown by students of Switzerland. Alpine valleys to Murchison (1851) were bays of an ancient sea; Schlaginweit (1852) found regional and local complicated crustal movements a satisfactory cause; and Forbes (1863) saw only glaciers.

The truths expounded by Desmarest and Hutton were reestablished by James D. Dana,[26] who in 1850 amply demonstrated that valleys on

[17]Wilson, J. W., Bursting of lakes through mountains: *Am. Jour. Sci.*, III, 253, 1821.

[18]Buckland, William, Reliquiæ diluvianæ: *Am. Jour. Sci.*, VIII, review, 150, 317, 1824.

[19]Hitchcock, Edward, Geology, mineralogy, and scenery of regions contiguous to the Connecticut River, with a geological map and drawings of organic remains (etc.): *Am. Jour. Sci.*, VII, 1-30, 1824.

[20]Phillips, John, Geology of Yorkshire: *Am. Jour. Sci.*, XXI, 17-20, 1832.

[21]Conrad, T. A., Notes on American geology: *Am. Jour. Sci.*, XXXV, 237-251, 1839.

[22]Darwin, C. R., Geological observations, etc., during the voyage of the "Beagle," London, 1844.

[23]Warren, G. K., Explorations in Nebraska and Dakota: *Am. Jour. Sci.*, XXVII, Review, 380, 1859.

[24]Lesley, J. P., Observations on the Appalachian region of southern Virginia: *Am. Jour. Sci.*, XXXIV, 413-415, 1862.

[25]For a fuller statement of the views regarding origin of valleys, see Gregory, H. E., Steps of progress in the interpretation of land forms: A century of science in America, pp. 124-152, Yale University Press, 1918.

[26]Dana, J. D., On denudation in the Pacific: *Am. Jour. Sci.*, IX, 48-62, 1850.

————, On the degradation of the rocks of New S. Wales and formation of valleys: *Am. Jour. Sci.*, IX, 289-294, 1850.

the Pacific islands owe neither their origin, position nor form to the sea or to structural factors, but are the work of existing streams which have eaten their way headwards. Even the valleys of Australia cited by Darwin as type examples of ocean work are shown to be products of normal stream action. Dana went further and gave a permanent place to the Huttonian idea that many bays, inlets, and fiords are but the drowned mouths of river-made valleys. The theory that valleys are excavated by streams which occupy them received strong support from study of the Rocky Mountain gorges (1862) and gained all but universal acceptance after Newberry[27] called attention to the lesson to be learned from the canyons of Arizona:

> Like the great cañons of the Colorado, the broad valleys bounded by high and perpendicular walls *belong to a vast system of erosion, and are wholly due to the action of water.* . . . The first and most plausible explanation of the striking surface features of this region will be to refer them to that embodiment of resistless power—the sword that cuts so many geological knots—volcanic force. The Great Cañon of the Colorado would be considered a vast fissure or rent in the earth's crust, and the abrupt termination of the steps of the table-lands as marking lines of displacement. This theory though so plausible, and so entirely adequate to explain all the striking phenomena, lacks a single requisite to acceptance, and that is *truth.*

With these stupendous examples in mind, the dictum of Hutton seemed reasonable: "There is no spot on which rivers may not formerly have run."

Contributions to physiography between 1850 and 1870 reveal a tendency to accept greater degrees of erosion by rivers, but the necessary end-product of subaerial erosion—a plain—is first clearly defined by Powell (1875),[28] who introduced the term "base level," which may be called the germ word out of which has grown the "cycle of erosion," the master key of modern physiographers.

Analysis of Powell's view has given definiteness to the distinction between "base level," an imaginary plane, and a "nearly featureless plain," an actual land surface, the final product of subaerial erosion. Following their discovery in the Colorado Plateau Province, denudation surfaces were recognized in Pennsylvania by McGee,[29] and in Connecticut by Davis (1889)[30] who introduced the term "peneplain," "a nearly featureless plain," for the upland of southern New England developed during Cretaceous time.

Long before the days of Powell "plains of denudation" had been clearly recognized by English geologists who considered them products of marine work. The contribution of American students is not that peneplains exist but that many of them are the result of normal

[27]Newberry, J. S., Colorado River of the West: *Am. Jour. Sci.*, XXXXIII, review, 387-403, 1862.

[28]Powell, J. W., Exporation of the Colorado River of the West, 1875.

[29]McGee, W. J., Three formations of the Middle Atlantic Slope: *Am. Jour. Sci.*, XXXV, 120, 328, 367, 448, 1888.

[30]Davis, W. M., Topographic development of the Triassic formation of the Connecticut Valley: *Am. Jour. Sci.*, XXXVII, 423-434, 1889.

subaerial erosion. More precise field methods during the past decade have revealed the fact that no one agent is responsible for the land forms classed as peneplains; that not only rivers and ocean, but ice, wind, structure, and topographic position must be taken into account.

The recognition of rivers as valley-makers and of the final result of their work necessarily preceded an analysis of the process of subaerial erosion. The first and last terms were known, the intermediate terms and the sequence remained to be established. Significant contributions to this problem were made by Jukes' (1862) discussion of "lateral" and longitudinal" valleys, Powell's description of antecedent and consequent drainage (1875), and Gilbert's analysis of land sculpture in the Henry Mountain (1880). But the master papers are by Davis,[31] who introduces an analysis of land forms based on structure and age by the statement:

> Being fully persuaded of the gradual and systematic evolution of topographical forms it is now desired . . . to seek the causes of the location of streams in their present courses; to go back if possible to the early date when central Pennsylvania was first raised from the sea, and trace the development of the several river systems then implanted upon it from their ancient beginning to the present time.

That such a task could have been undertaken only three decades ago and today be considered a part of every-day field work shows how completely the lost ground has been regained and how rapid has been the advance in the knowledge of land sculpture since the canyons of the Colorado Plateau were interpreted.

One of the most interesting chapters in geological history is the origin and development of the theory of continental glaciation, which grew out of the attempt to explain the presence of "erratic" bowlders strewn over the surface in "obviously unnatural" positions. As stated by Silliman (1821):[32]

> The almost universal existence of rolled pebbles, and boulders of rock, not only on the margin of the oceans, seas, lakes, and rivers; but their existence, often in enormous quantities, in situations quite removed from large waters; inland,—in high banks, imbedded in strata, or scattered, occasionally, in profusion, on the face of almost every region, and sometimes on the tops and declivities of mountains, as well as in the valleys between them; their entire difference, in many cases, from the rocks in the country where they lie—rounded masses and pebbles of primitive rocks being deposited in secondary and alluvial regions, and vice versa; these and a multitude of similar facts have ever struck us as being among the most interesting of geological occurrences, and as being very inadequately accounted for by existing theories.

To this list of features now recognized as characteristic of glacial drift are to be added jumbled masses of "diluvium," ridges of gravel, "kettles" in sand plains, polished and striated rock, and thick beds of

[31]Davis, W. M., The rivers and valleys of Pennsylvania: *Nat. Geog. Mag.*, I, 183-253, 1889.

———, The rivers of northern New Jersey with notes on the classification of rivers in general, ibid, II, 81-110, 1890.

[32]Silliman, Benjamin, Review of Hayden's geological essays: *Am. Jour. Am. Jour. Sci.*, III, 49, 1821.

"unhardened pudding stone" (till). Even Lyell, the great exponent of uniformitarianism, appears to have lost faith in his theories when confronted with facts for which known causes seemed inadequate.

The interest aroused by the phenomena now attributed to ancient glaciers is attested by scores of titles in scientific and literary periodicals of the first four decades of the nineteenth century. With little knowledge of existing glaciers, of areal distribution, structure and composition of drift, all known forces were called in: weathering, catastrophic floods, ocean currents, waves, icebergs, glaciers, wind, and deposition from a primordial atmosphere. Even human agencies were not discarded. But the controversy ranged chiefly about floods, icebergs, glaciers, and earth shaking catastrophes.

The catastrophes favored by most geologists were the Deluge, and floods of water violently released from the interior of the earth or caused by sudden upheaval of mountains. "We believe," says Silliman (1824) "that all geologists agree in imputing . . . the diluvium to the agency of a deluge at one period or another"[33]—a conclusion which rested in no small way upon Hayden's[34] well-known treatise on "diluvium" (surficial deposits, glacial drift). The objection to the theory of "debacles" and resistless world-wide currents is not only its grotesque assumptions and processes but also its complete disregard of observable phenomena. Its strength lay chiefly in its supposed confirmation of the Biblical record and it is perhaps natural that the way to a saner view should have been pointed out by intelligent laymen rather than by leaders of thought bound by authority and tradition. Unbiased observation is an essential condition of progress.

In 1823[35] Granger speaks of the glacial striæ on the shore of Lake Erie as

> having been formed by the powerful and continued attrition of some hard body. . . To me, it does not seem possible that water under any circumstances, could have effected it. The flutings in width, depth and direction, are as regular as if they had been cut out by a grooving plane. This, running water could not effect, nor could its operation have produced that glassy smoothness, which, in many parts, it still retains.

The first unequivocal statement that ice is an essential factor in the formation and transportation of drift comes from another layman, Peter Dobson (1826),[36] who concludes a series of accurate detailed observations on the polished and striated bowlders embedded in the Connecticut till with the remark:

> I think we cannot account for these appearances, unless we call in the aid of ice along with water, and that they have been worn by being suspended and carried in ice, over rocks and earth, under water.

[33]Silliman, Benjamin, Review of Hayden's geological essays: *Am. Jour. Sci.*, VII, 211, 1824.

[34]Hayden, H. H., Geological essays, 1-412, 1821: *Am. Jour. Sci.*, III, 47-57, 1821.

[35]Granger, Ebenezer, Notice of a curious fluted rock at Sandusky Bay, Ohio: *Am. Jour. Sci.*, VI, 180, 1823.

[36]Dobson, Peter, Remarks on bowlders: *Am. Jour. Sci.*, X, 217-218, 1826.

The glacial theory makes its way into geological literature with the development by Agassiz (1837) of the views of Venetz (1833) and Charpentier (1834) that the glaciers of the Alps once had greater extent. The bold assumption was made that the surface of Europe as far south as the shores of the Mediterranean and Caspian seas was covered by ice during a period immediately preceding the present. The kernel of the present glacial theory is readily recognizable in these early works, but it is wrapped in a strange husk: the Alps were assumed to have been raised by a great convulsion under the ice and the erratics to have slid to their places over the newly made declivities. The publication of the famous "Etudes sur les Glaciers" (1840), remarkable alike for its clarity, its sound inductions, and wealth of illustrations, brought the ideas of Agassiz into prominence and inaugurated a thirty years' war with the proponents of floods and of icebergs. The outstanding objections to the theory were the requirement of a frigid climate and the demand for glaciers of continental dimensions; very strong objections for the time when fossil evidence was not available, the great polar ice sheets unexplored, and the distinction between till and water-laid drift had not been established.

So fully does the glacial hypothesis account for observed phenomena that it received the sympathetic attention of leading geologists especially in America. As the evidence accumulated opposition disappeared and by 1875 the belief in the former wide extent of land ice was firmly established.[37] The next step forward was the determination of the extent of glacial drift—a series of field studies that have produced the modern maps of glaciated areas and led to the interesting conclusion that the "ice age" was not the record of the advance and retreat of one great continental glacier, but that it is divided into epochs; that several retreats are required to account for the phenomena of buried soils and overlapping ice laid deposits. In 1883 Chamberlin[38] presented his views, under the bold title "Preliminary Paper on the Terminal Moraine of the Second Glacial Epoch," which initiated the discussion that led to the recognition of glacial deposits of different ages and the features of interglacial periods. Field studies during the last quarter century have demonstrated five glacial stages in America and four in Europe.

Within the present generation sculpture by glaciers has received much attention and has involved a reconsideration of the ability of ice to erode which in turn involves a crystallization of views of the mechanics of moving ice. The inadequacy of structural features or of

[37]Gregory, H. E., Steps of progress in the interpretation of land forms: A century of science in America, pp. 122-152, Yale University Press, 1918.

[38]Chamberlin, T. C., Preliminary paper on the terminal moraine of the second Glacial period: *U. S. Geol. Survey, Third Ann. Rept.*, pp. 291-402, 1883.

river corrasion to account for flat-floored, steep-walled gorges, hanging valleys, and many lake basins, has led to the present fairly general belief in the long neglected views of Ramsay that glaciers are powerful agents of rock sculpture. The details of the process, particularly of cirques, are not yet fully understood.

Meaning of Fossils

From the time when fossils received general recognition as the remains of extinct organisms, they have been examined from two viewpoints. One group of students (stratigraphers) are interested in fossils as objects which characterize geological epochs and by means of which true succession and relative ages may be determined. The other group (paleozoologists and paleobotanists) find the supreme value of fossils in their bearings on the problems of origin, development, and evolution of living forms. It is this biological aspect which has aroused an almost universal interest in fossils, brought the teachings of geology into zoological laboratories and medical schools, and furnished material for controversy to theologians and philosophers. The founders of paleontology, Blumenbach (1803-1816), Schlotheim (1804), Sternberg (1804), Cuvier (1808), Lamarck (1815-1822), and Brongniart (1822), attained success by applying the methods of comparative anatomy and botany, and the subject found an assured position through the work of Buckland (1836), Mantell (1844), Pictet (1844-1846), Geinitz (1846), Quenstedt (1852), and Richard Owen (1860)—all primarily biologists. Vertebrate paleontology especially has been treated as a branch of comparative anatomy concerned primarily with fossil bones and teeth, but its contributions have brought the civilized world to a belief in the theory of organic evolution.

Fossils were correctly considered by the Greeks and Romans as remains of plants and animals, but their presence in the rocks was ascribed to gigantic inundations which had brought marine animals far inland. Avicenna (980-1037), the great Arabian scholar, thought fossils were the unfinished work of *vis plastica*, a creative force that changed inorganic substances to organic; the living form had been produced but no life given it. To George Bauer [Agricola] (1494-1555), and to Mattioli (1548) fossils were "solidified accumulations from water" like limestone, or converted into stone by a certain *succus lapidescens* believed to reside in water; to the anatomist Fallopio fossil teeth were concretions and fossil shells the result of "fermentations" and "exhalations from the soil"; to Olivi of Cremona they were mere sports or freaks; Lister (1638-1711) taught that each rock stratum produces its own fossils; Mercati, museum assistant to Pope Sixtus V, thought them seeds of the stars; and the English antiquary

Lhuyd (Luidius) sought their origin in seed-bearing vapors originating in the sea. These typical seventeenth century ideas of the nature of fossils are to be contrasted with those of Leonardo da Vinci (1452-1519) and Fracastora (1483-1553) who insisted that fossils are organisms which once lived where now found, and which owe their preservation to burial in mud.

The conclusions of these Italian scholars who ridiculed the notion that fossils descended from stars or were formed in the earth by some mysterious creative force were disregarded or treated as "vaporings of disordered minds"; Bernard Palissy (1499-1589) who near the close of the sixteenth century gave a correct explanation of petrified wood, fossil fish and molluscs was vigorously denounced as a heretic. Even the teachings of the remarkable scholar, Nicholaus Stensen (Nicolas Steno, 1631-1687), whose little pamphlet *"De solido intra solidum naturaliter contento"* (1669) is the high water mark of seventeenth century geology, made little impression and was soon forgotten, and at the beginning of the eighteenth century fossils were generally considered mineral curiosities—"formed stones," "figured stones"—and chance imitations of living forms.

Fortunately the disputes regarding the nature of fossils encouraged the search for fossils and led to a number of valuable works in which fossils were faithfully described and represented by drawings. Publications descriptive of fossils of particular regions, monographs on selected groups, and general treatises on classification and nomenclature appeared in France, England, Germany, Switzerland, and Italy during the early part of the eighteenth century. Through the labors of Scheuchzer[39] and many supporters, Johann Baier[40] and especially John Woodward,[41] and Knorr and Walch,[42] whose handsome four volume treatise is the paleontological masterpiece of that period, trilobites, brachiopods, molluscs, crinoids, sponges, crabs, fishes and vertebrate bones were made known to the scientific world. The accumulated evidence was conclusive and at the middle of the eighteenth century no scholar of repute looked on fossils as the result of inorganic forces.

With the recognition of fossils as the remains of living beings, the three century discussion of the origin of fossils assumed new form. Are these objects the *relictae* of animals and plants now living or do they represent peculiar races of animals and plants which formerly inhabited the earth? Have they originated where found or have they been transported to their present resting places, and if transported,

[39]Scheuchzer, J. J., Specimen lithographie helveticae curiosae, 1702.

[40]Baier, Johann, Oryctographica norica, 1712.

[41]Woodward, John, Natural history of the earth and terrestrial bodies, etc., London, 1695.

[42]Knorr, G. W., and Walch, J. E. F., Die Sammlung von Merkwürdigkeiten der Natur und Alterthümer des Erdbodens.

by what agency? With the fauna of half the earth's surface and the life of the ocean unknown it was but natural to assume that fossil snails and oysters and leaves belonged to species of animals and plants which still flourished in some unexplored part of the world. It was commonly believed that the only animals in existence were those made during the days of creation and that none had disappeared from the world. Thus the bones of the ground sloth (*Megalonyx jeffersoni*) described by Thomas Jefferson were believd by him to be the remains of some sort of a lion still living in the Allegheny Mountains. But the hope of finding living specimens to match the skeletons embedded in the rock resulted in disappointment and in the search for other explanations the theory of great catastrophes which overwhelmed the inhabitants of all or parts of the earth gained the support of the leading minds toward the close of the eighteenth century. Great inundations of the sea, terrific earthquakes, and gigantic volcanic eruptions all had their supporters, but the belief in Noah's flood enlisted the most faithful adherents. The biblical flood not only swept the earth of living forms, but scattered their remains far and wide and left them buried in jumbled heaps in the sands and muds deposited by the onrushing currents. Warmly approved by the church, the "diluvialists" occupied a strong position in the scientific world well into the nineteenth century. Even the great Cuvier (1821) lent support to the believers in the flood and Buckland's treatise on the *Organic Remains contained in Caves, Fissures and Diluvial Gravel, and on other phenomena Attesting the Action of a Universal Deluge,* bears the date 1823.

With a wider recognition of the fact that fossils are not restricted to sands and gravels and muds which might have been deposited within the past few thousands of years, but are found embedded in firm rock on plains and seashore and mountain tops and are revealed by mine shafts, wells, tunnels, and excavations for buildings, the diluvial hypothesis assumed yet another form. Noah's flood was retained, but was given the position of the last of a series of great catastrophes which overwhelmed the world.

Under the lead of the French paleontologists, cordially supported by their English and American colleagues, the "catastrophists" held sway during the first six decades of the nineteenth century. They clearly recognized that fossils in a given formation differed in kind from those in the overlying and underlying strata, but explained these facts on the theory that the period represented by each of these formations witnessed the complete disappearance of animal and plant life of the world. The fossils of the next higher strata were the remains of newly created beings. Each species was a separate creation. The simplicity of forms of the earlier creations compared with the complexity of form and structure of the fossils of later creations appears

to have been ascribed to progressive skill of the Creator rather than to the progressive development of species.

Cuvier, the leader of the catastrophic school, is the outstanding figure among the paleontologists of the first half of the nineteenth century. As a biologist he established comparative anatomy as a distinct branch of science and formulated the principles and methods still in vogue for the study of fossil vertebrates. Through his influence systematic research replaced disorganized observation. His conception of the correlation of parts, that structure and function are interdependent, is the guiding principle in modern paleontology, and makes it possible to reconstruct an extinct animal from fragmentary remains found in the rocks or even from a single bone or tooth. His work shows a progression from description of individual bones to reconstruction of whole skeletons, and on to the grouping of extinct forms into species, genera, and orders. The wealth of fossil material embedded in the gypsum deposits of the Paris Basin "enabled him to prepare the first reconstructions of fossil vertebrates ever attempted and to bring before the eyes of his contemporaries a world peopled with forms which were utterly extinct."[43] To bring to the laboratory a miscellaneous assemblage of fossil bones and by the strict application of scientific method supply the missing parts until there appears an animal never before seen by human eye, may be considered one of the great achievements of the human mind. Little wonder that Cuvier's demonstrations revolutionized the thought of his day and made a deep and lasting impression. Paleontological views before the days of Darwin were essentially the views of Cuvier and his devoted disciples. Most of the epoch-making contributions of the Cuvierian school have remained undisputed but the contention that species are immutable is strangely out of harmony with modern views.

When the Cuvierians left the solid ground of their field of comparative anatomy they parted company with contemporary thinkers in other branches of geology and entered the bog already thickly populated with philosophers, theologians, and mystics of ancient and medieval times. Unconsciously and with different terminology, they gave their approval to Indian, Egyptian, and early Church beliefs in earth catastrophes followed by recreations—periods of disaster interspersed with millenia. There was no recognition of the orderly development of the earth and its inhabitants resulting from the operation of natural laws.

The publication of Darwin's "Origin of Species," 1859, marks the beginning of the evolutionary period in the study of fossils. The fixity of species was replaced by the evolution of species; recurrent

[43]Lull, R. S., On the development of vertebrate paleontology: A century of science in America, Yale University Press, 1918, p. 219.

catastrophes which necessitated new creations retired in favor of orderly development; and supernatural agencies were discarded. This revolutionary change in thought was foreshadowed by the teachings of a few bold spirits and the transition from catastrophism to evolution made easier by evidence accumulated during previous decades.

By 1856, two thousand fossils from strata later than the Carboniferous were known in America; in Europe more than 20,000. A study of this material led to the recognition of the facts that the individuals that compose a species are "endlessly diverse" (Dana); "that fossils from two consecutive formations are far more closely related than are the fossils of two remote formations" (Asa Gray); "that when species are arranged in a series and placed near to each other with due regard to their natural affinities they each differ in so minute a degree from those next adjoining that they almost melt into each other" (Lyell). And during the catastrophic period men were not lacking who accepted the evidence of transition in the organic world and followed it to its logical conclusion. Aristotle's views are singularly like those of modern time and Erasmus Darwin (1731-1802), grandfather of Charles Darwin, consistently taught that variations in species arise within organisms in response to environmental influences. Comte de Buffon (1707-1788) grasped the idea that life descends continuously from other life and is modified by geographical isolation, but only the industrious and serious-minded can separate the wheat from the chaff in the 44 volumes of his entertaining Histoire Naturelle (1749-1804).

Among evolutionists of Pre-Darwin days, Chevalier de Lamarck 1744-1829) stands first. For fifty years he was a firm believer in catastrophes and recreations but in later life, in the face of strong opposition, he gave the full weight of his knowledge and experience to the support of the theory of descent and inheritance of acquired characters. His teachings are so unmistakably clear and so sharply contrasted with the contentions of the catastrophists that Lamarck is justly regarded as the founder of the evolutionary school. Lamarck's ideas were kept alive by a group of earnest but unconvincing followers including Geoffroy Sainte-Hilaire and the poet Goethe, but such men were no match for the gifted scientists of the catastrophic school, supported as they were by the church and by public opinion. Even the *Vestiges of the Natural History of Creation* by Robert Chambers (1802-1871), the most discussed book of the time, failed to uproot traditional beliefs, and by 1850 the evolutionary theory was pronounced "dead" by the leading writers of the time.

The resurrection came with the publication of Darwin's *Origin of Species*, doubtless the most influential book of the nineteenth century. No wonder that Darwin's views were received with dismay and

aroused strenuous and bitter opposition, for their acceptance gave the death blow to creationists, placed man among the animals, and otherwise undermined the supposedly plain teachings of Scripture. The theory early received the support of Hooker, Huxley, and Herbert Spencer in England, and Asa Gray in America. Among its American opponents were James D. Dana, who later modified his opinion, and Louis Agassiz, who held his disapproval through life. That part of Darwin's theory which related to the progressive development of *living* plants and animals aroused little opposition, for improvements produced by the breeding of domesticated animals were well understood; but the testimony of the rocks that the lineal ancestors of existing animals are constituents of strata laid down millions of years ago was quite another matter.

The scientific opponents of evolution relied mainly on the fact, uncontested by geologists, that the successive strata did not disclose an unbroken series of modified forms—there were many "missing links" in the supposed chain of development. In this connection the discoveries of American vertebrate paleontologists make an interesting chapter. Beginning with 1870, Leidy, Cope, Marsh, followed by a group of workers of the present generation, unearthed the profusion of vertebrate remains from Tertiary, Cretaceous, and Jurassic beds which have made famous the collections in the American Museum of Natural History and the Peabody Museum at Yale. Professor Marsh alone found the remains of about 200 birds with teeth, 160 mammals, and hundreds of flying, swimming, and walking reptiles varying in size from guinea pigs to monsters 80 feet long. These collections bridged the gap between reptiles and birds and indicated the common ancestors for animals now recognized as distinct species.

Down through successive geological epochs the modern horse was traced through transitional forms to a four-toed ancestor, the size of a fox, which flourished during the Eocene. Such evidence could not be disregarded. In reviewing the work of Marsh, Huxley who previously had pointed out the insufficiency of the paleontological evidence, declared that "the evolution of existing forms of animal life from their predecessors is no longer an hypothesis but an historical fact" (1876).

Like other animals of the modern world, man's ancestry has been traced far back. The discovery of human bones and implements intermingled with the remains of animals long extinct proved a human habitation in France (Abbeville); Germany (Neanderthal: Fuhlrott 1857); and England (Piltdown: Woodward 1913) during Pleistocene time, and in Java (Do Bois 1891) at perhaps an even earlier date.

The Geologic Time Scale

Since the beginnings of field observations it has been known that many rocks are arranged in layers and that in many places strata of

different colors and texture and composition are piled one upon another in a regular series. But nearly 18 centuries elapsed before it was realized that the stratified rocks contain within themselves the evidences of their origin and reveal a record of alternating lands and seas, of volcanic outpourings and desert winds, of changing climates and surface forms. A yet longer time was required to grasp the stupendous truth that the history of life on the earth is to be deciphered from the organic remains embedded in the hardened sediments.

The true meaning of chronological sequence, the recognition of the fact that the debris of lower strata has been utilized in building the strata next above, was first made clear by Arduino (1713-1795) who separated the rocks of Northern Italy into Primitive, Secondary, Tertiary, and Volcanic (1759). The field methods and manner of presentation developed by Arduino are not unlike those employed today and entitle this pioneer worker to a prominent place among stratigraphers.

An advance is shown in the work of Füchsel (1722-1773)[44] who analyzed the sedimentary masses of Thuringia. By his painstaking field mapping, his insistence that groups of strata have definite chronologic value, and especially by his clear distinction of stratum (Schicht) and formation (*Series montana*), were laid the foundations of stratigraphic geology in Germany.

The high priest of stratigraphy for the eighteenth century was Abraham Gottlob Werner (1749-1817), professor in the School of Mines of Freiberg—the first geologist to obtain world-wide prominence. Werner's contributions to literature are of small importance: his strength lay in his familiarity with the geological researches of his time and even more in his remarkable ability in teaching which made of Freiberg the Mecca for European students. Based on the conception of universal formations as developed by Füchsel, and on the systematic arrangement of minerals as outlined by the Swedish mineralogist, Tobern Bergman,[45] Werner erected the study of rock formations into an independent branch of geology. The essence of his teaching lies in the view that all rock formations are world-wide and that all are chemical precipitates; that the world is like an onion to which successive layers have been added. He conceived of a primeval ocean completely enveloping the earth. From this shell of water were precipitated first the granites and associated green stones, hornblende schists and porphyries, then slates and greywackes, followed in turn by limestone, coal, basalt, and ores; by sand, clay, soapstone, and finally by volcanic ash, some lavas, and jasper. Obviously, all igneous

[44]Füchsel, G. C., Historia terræ et maris ex historia Thuringiæ permontium descriptionem erecta (*Acta Acad. elect. Moguntinae*), 1762.
————, Entwurf zur ältesten Erd und Menschen Geschichte, 1773.
[45]Bergman, Tobern, Physical description of the globe, 1766.

and metamorphic rocks found place with the sediments for there was no place in this scheme for igneous activity, nor for structural changes in the earth's crust. To Werner volcanoes were "burning mountains" —the evidence of spontaneous combustion of buried beds of coal precipitated by an ancient sea. To him the world is the handiwork of Neptune; Pluto was disregarded. In spite of fundamental errors Werner's teachings were dominant to the close of the eighteenth century, and in the early nineteenth century had the backing of leading scholars of Europe, and guided the work of Maclure, Eaton and Silliman, the first American geologists. Only after a contest lasting for two decades did the opponents of the Wernerian School succeed in establishing the difference between igneous masses and sedimentary rocks in the geological series.

This prolonged discussion greatly stimulated observation and encouraged attempts at subdivision of stratified rocks which, however, showed little improvement over the work of Arduino and Füchsel. Progress depended on the development of new methods. The man and the method appeared in an unexpected place. William Smith, a civil engineer (1769-1838), had been quietly at work in all parts of England noting the position, extent, and composition of sedimentary rocks, collecting fossils from each stratum, and recording his observations on colored geological maps and sections. As part of his daily routine Smith noted that certain fossils reappear in the same beds at different localities and that each fossil species is entombed in a definite formation. From this he drew the obvious inference that sedimentary formations may be recognized by their fossil content, and showed that one succession of sediments extends across England from south to east. In this matter-of-fact way the sure foundations of modern stratigraphy were laid; a modest lover of nature had found the way to read the history of the earth—one of the truly great contributions to science. Before William Smith, stratigraphic position and geologic age were based on chemical and mineralogical composition and attitude of rocks; fossils were incidental. Since his day fossils are the final court of appeal for questions of time and order of succession, and correlation of widely separated beds. The adequacy of Smith's methods was amply demonstrated in Conybeare and Phillips' "Outlines of the Geology of England and Wales" (1822)[46] and with the wide distribution of Lyell's famous "Principles of Geology" (1830-1833) came universal recognition of the fact that fossils provided the surest means for comparative study of sedimentary rocks.

Primitive, Transition, Secondary, Tertiary—the recognized subdivisions of the first quarter of the nineteenth century—gradually gave

[46]Conybeare and Phillips' "Outlines of the geology of England and Wales" was the first widely used treatise in the English language.

way to the ages and systems of the modern time-scale. In 1830 the three divisions of the Tertiary based on relative percentage of existing species were established by Deshayes after a comparative study of the Tertiary rocks of England, France, Belgium, Poland, Hungary, and Italy. Lyell (1833) gave them the names now in use: Eocene, Miocene, Pliocene, and later added the term Pleistocene for the most recent alluvium and for the deposits now classed as glacial drift. The equivalents of certain English formations described by Smith were recognized in the Jura Mountains and in 1829 given the name Jurassic. With the addition of Triassic in 1834, the earlier "Secondary Class" became Triassic, Jurassic, and Cretaceous periods of Mesozoic (medieval) time. The analyses of the "Transition Class" of Werner and his contemporaries began with setting limits to the Carboniferous (1821) and continued through the establishment by English workers of the Cambrian (1833), the Silurian (1835), the Devonian (1839), and the Permian (1841) as periods of Palaeozoic time. Even the "Primitive or Primary Class," supposed by the earlier stratigraphers to be the veritable bed-rock of the earth, was resolved by Logan into Huronian (1852) and Laurentian (1853) systems as periods of Archaean (Pre-Cambrian) time. It thus appears that during the forty years following the publication of Smith's geological memoir, English geologists had developed a time-scale by which the relative age of all the sedimentary and igneous rocks of the world could be measured.

Stratigraphic research during the second half of the nineteenth century has added volumes to the history of the earth. The increase in the number of gathered fossils from thousands to tens of thousands permits closer discrimination of horizons and with added knowledge of the breaks in the sedimentary record has led to a recognition of subdivisions in the Silurian, Carboniferous, and Cretaceous. By far the greatest contributions during the past half century came from America. Through state and federal surveys,[47] and university activities, the condition of the earth during Cretaceous, Triassic, and Carboniferous times has been written into the record, and the work of James Hall[48] and his associates has made the stratified rocks of New York State the standard Paleozoic section for the world. American stratigraphers like American paleontologists have advanced from learners to teachers.

During the past quarter century attention has been directed to determining the physical conditions surrounding the deposition of sediments with a view to picturing more clearly the distribution of seas and lands, of streams and mountains, and separating areas of erosion from regions of deposition. The history of climates is also receiving atten-

[47]Government geological surveys: A century of science in America, pp. 193-216. Yale University Press, 1918.

[48]Zittel, Karl von, History of geology and paleontology, p. 442, 1901.

tion and one of the most striking results of modern methods is the proof of glacial conditions not only in the Pleistocene but in the Permian of India, Africa, Brazil, Australia, and Massachusetts, and even among the oldest rocks of China, Norway, and Canada.

It is thus seen that fossils in a modern sense are more than proofs of evolution and more than markers which indicate relative age. They aid in writing the physical geography of the time in which they flourished.

If the record were complete enough it should be possible to locate the seas and lands, the lakes and rivers, reconstruct the mountains and plains, and restore the inhabitants of each geological period. Even with the meager fossil record, geologists are drawing coast lines of the earliest lands, pointing out deserts where rainfall is now abundant and marking ancient tropical seas where cold winters now prevail. One of the most promising developments of the twentieth century is the preparation of physical geographies of important geological eras under the joint authorship of stratigraphers, paleontologists and physiographers.[49]

Age of Earth

The attractive myths of earth origin formulated by most uncivilized races wisely refrain from giving quantitative values to the expression "long ago." The philosophers of India regarded the earth as eternal; the Chaldeans set 2,150,000 years as the age of the earth; Zoroaster was satisfied with 12,000 years, and with the establishment of Christianity in Europe, the Hebrew chronology prevailed and the limiting dates of earth history rested firmly on the recorded teachings of Moses. The views of the Christian world well into the nineteenth century were fairly represented by Bishop Ussher who in 1650 fixed the date of the creation of the earth at 4004 B. C. Strangely enough this figure founded on no facts and no arguments rose to the dignity of a doctrine. For 200 years it appeared on the margin of our English Bibles and was the test of orthodoxy. Even today this date or the corresponding Byzantine date 5509 B. C. is accepted by half of the Christian world. Under the influence of these ideas geologists up to the beginning of the nineteenth century felt compelled to squeeze all geological history into six or seven thousand years. This severe restriction could be harmonized with the growing body of geological fact only by the formulation of extraordinary hypotheses, a state of affairs that led to the magnification of Noah's flood and similar catastrophes as the all powerful agents in molding the surface of the earth.

The scientific world was released from this thraldom by the bold teachings of Hutton that the present slow rate of geological processes must have been the rule since the dawn of geologic history. No wonder

[49]Schuchert, Charles, The progress of historical geology in North America: A century of science in America, pp. 60-121, Yale University Press, 1918.

that this view encountered opposition; it appeared to shake the very foundations of Christianity. The adherents to the established Church chronology had scarcely recovered their breath when Darwin's "Origin of Species" brought from the biological realm data in support of the physical evidence developed by Hutton, Lyell, and others. The theory of evolution obviously demanded enormous drafts on time and was utterly inconsistent with previously accepted views. By the third quarter of the nineteenth century the conclusions of geologists and paleontologists had become too well grounded to permit of substantial modification, but happily the first three words of the Bible, "In the beginning," and the "days" of creation were subject to new interpretations and the smoke of battle cleared away.

During the last half of the nineteenth century the advocates of a very ancient earth found themselves out of accord with the teachings of physics. Under the leadership of Lord Kelvin, mathematical proof was presented that the sun could not have been giving out heat for more than 100,000,000 years, perhaps only 40,000,000, and since the sun must have been producing heat for untold millions of years before life could have existed on the earth, only 10,000,000 to 20,000,000 years could be allowed for geological history. This amount of time is altogether too short for known geologic processes and for the evolution of living forms. When it is realized that the Cretaceous period alone may have had a duration equal to that allowed by Kelvin for all geologic time, the inadequacy of the physical estimates is apparent.

Though viewed with suspicion, the physical evidence appeared for a time irrefutable. Darwin was led to abandon his figures and some geologists undertook to speed up geological processes. In 1895 a re-examination of the physical data by Professor Perry revealed the weakness of Kelvin's arguments and modern students of radio-activity give the geologists not only the one or two hundred millions of years for which they have been contending, but allow 185,000,000 years since Carboniferous time and more than a billion years since the earth's first rocks were formed.[50]

The history of the earth as written during the past century is a fascinating story which has profoundly affected the world's thinking. Some chapters are complete, some need revision, many remain to be written. The interior of the earth and half of the surface of the earth await geological exploration; the mechanics of earth movements are not understood; the causes of variation in climate are imperfectly known; and the origin of life on the earth is shrouded in mystery. The chief problems awaiting solution call for assistance from chemistry, physics, biology, and astronomy, and further advance involves sympathetic coöperation.

[50]Holmes, Arthur, Age of the earth, Harper and Brothers, New York, 1912.

IS DARWIN SHORN?

By Professor C. C. NUTTING
STATE UNIVERSITY OF IOWA

NO one likes to criticise such a man as John Burroughs. His charming portrayal of the ways and manners of our birds and chipmunks and other "wee beasties" has endeared him to all of us. We who are naturalists are grateful for his keen observation and accurate descriptions, and most of all for his refusal to be lured into the path of the nature faker that has led to the discrediting of some of our most promising writers on natural history subjects.

But the more he is admired and respected, the greater his following among lovers of good literature, the subtler his power to mislead when he goes astray. This he certainly does in his paper "A Critical Glance into Darwin," which appeared in the August number of *The Atlantic Monthly*.

In the very first paragraph he says: "It is with Darwin's theories that I am mainly concerned here. He has already been shorn of his selection doctrines as completely as Samson was shorn of his locks."

This is a rather serious statement to be made in an off-hand manner; moreover it is far from true. I believe that I have a sufficiently wide acquaintance with working naturalists of the present time and a sufficient knowledge of their attitude towards the theory of natural selection to justify this rather blunt expression.

De Vries regarded his own mutation theory as a contribution to the theory of Natural Selection. Jennings, one of our foremost protozoologists says, "Evolution according to the typical Darwinian scheme, through the occurrence of many small variations and their guidance by Natural Selection, is perfectly consistent with what experimental and paleontological studies show us; to me it appears more consistent with the data than does any other theory." Castle, one of our leading geneticists, believes in continuous variations not following a single necessary trend, but guided by selection; and E. B. Wilson, among the most honored of all American biologists, says: "And yet, as far as the principle is concerned I am bound to make confession of my doubts whether any existing discussion of the problem affords more food for reflection, even today, than that contained in the sixth and seventh chapters of 'The Origin of Species' or elsewhere in the works of Darwin."

The chapters referred to are captioned "Difficulties of the Theory" and "Miscellaneous Objections to the Theory of Natural Selection." Here Darwin has gathered together all of the criticisms that had appeared up to the edition of 1896, twenty-eight years after the first publication of the theory, and has answered them with the utmost fairness and candor.

In this connection I can not refrain from quoting from my revered teacher, Dr. David Starr Jordan. Speaking of the "Origin," he says: "There is in it no statement of fact of any importance which, during the twenty-five years since it was first published, has been proved to be false. In its theoretical part there is no argument which has been proved to be unfair or fallacious. In these twenty-five years no serious objection has been raised to any important conclusion of his which was not at the time fully anticipated and frankly met by him."

No one can dispute the authoritative position of these men among American zoologists. Numerous others could be cited, and I am confident that none of them would subscribe to the statement that Darwin's selection doctrine has been overthrown. More than this; there is no doubt in my mind that if the entire membership of the American Society of Zoologists could express their opinion an overwhelming majority would assert that natural selection as proposed and elaborated by Charles Darwin is still the best explanation of organic evolution. No discarded theory could command so great a following among professional naturalists as does this.

Mr. Burroughs objects strenuously to Darwin's statement that he saw one of the chief factors of evolution in fortuitous or chance variations, and declares: "I can no more think of the course of evolution as being accidental in the Darwinian sense than I can think of the evolution of the printing press or the aeroplane as being accidental, although chance has played its part."

Now what is "chance" in the Darwinian sense? Darwin himself gives an explicit answer at the beginning of his chapter on Variation: "I have hitherto sometimes spoken as if the variations so common and multiform * * * were due to chance. This is, of course, a wholly incorrect expression, but it serves to acknowledge plainly our ignorance of the cause of each particular variation." Nothing could be more honest and straightforward than this; and it is manifestly unfair to accuse Darwin of using "chance" as if it meant lawless or opposed to law. The friends of Darwin have a right to insist that fair-minded critics refrain from this sort of misrepresentation.

The majority of present day zoologists and botanists do not talk much about Darwinism or Natural Selection; not because they do not believe these things but because they regard them as virtually settled and have turned to other matters, particularly the mechanism

of heredity and the causes of variation, matters which Darwin did not explain satisfactorily because knowledge of these has been almost entirely the result of post-Darwinian investigation. At present most biologists believe in Mendelism and many of them have accepted as true the mutation theory. For all of these the chromosome looms so large as to obscure most of their mental horizon. I have seen a twenty-foot chart of a chromosome fully eclipse a map of the world, and thought it emblematic of the present situation.

It seems evident that a large number of our younger generation of biologists have fallen into strange confusion owing to a lack of appreciation of the difference between "variation" as used by the mutationist and the same word as used by Darwin. The mutationist uses the word in the sense of fluctuating varieties as opposed to mutations, while Darwin did not so restrict its use and, as will be shown, included what are now known as mutations in his "individual variations." De Vries, the father of the mutation theory, was entirely without mental confusion of this sort, and I can best make my point by quoting him. He says:

"Darwin discovered the great principle which rules the evolution of organisms." In discussing the steps by which progress is made he says: "On this point Darwin has recognized two possibilities. One means of changes lies in the sudden and spontaneous production of new forms from the old stock. The other method is the gradual accumulation of those always present and ever fluctuating variations which are indicated by the common assertion that no two individual of the same race are exactly alike. The first changes are what we call 'mutations,' the second are designated as 'individual variations' or, as this term is often used in another sense, as 'fluctuations.' *Darwin recognized both lines of evolution.*" The italics are mine.

What I am driving at is this. Many mutationists have become so accustomed to the word "variations" as synonymous with "fluctuations" that they assume that Darwin used the word exclusively in that sense when he speaks of individual variation as one of the main factors in natural selection. They therefore erroneously conclude that the mutation theory is squarely opposed to natural selection and in adopting the former suppose that they thus abandon the latter.

II

The work of DeVries, Morgan, Castle and a host of other men who have gone deeply into the mechanism of heredity and have been particularly interested in the behavior of the chromosomes as expressed by the Mendelian Law, has shown very conclusively that a majority of mutations are exceedingly minute, and that they range from hardly detectable changes to the much rarer great mutations recorded of the evening primrose. The difference between fluctuations

and mutations is not quantitative but qualitative, the latter only being inheritable although they may be quantitatively no more conspicuous than the slightly different color of the eyes of *Drosophila*.

Now we must remember that all slight variations, be they mutations or fluctuations, were simply "individual variations" in Darwin's time, and both were included in his term "variations." In speaking of variability, he says: "In all cases there are two factors, the nature of the organism (heredity) *which is by far the most important of the two*, and the nature of the conditions (environment)." The words within parentheses and those in italics are mine. It is quite evident that the modern geneticist would interpret variations due to the former of these "factors" as mutations and those due to the latter as fluctuations, and that Darwin distinguished between the two so far as the knowledge available in his time permitted. He also plainly regards the mutations as much the more important.

De Vries, therefore, had keener vision than some of his followers when he said: "My work claims to be in full accord with the principles laid down by Darwin and to give a thorough and sharp analysis to some of the ideas of variability, inheritance, selection and mutation which were necessarily vague in his time." It seems quite certain that the founder of the mutation theory did not regard Darwin as "shorn of his selection doctrines."

III

In 1903 Professor T. H. Morgan published his "Evolution and Adaptation" containing the most elaborate criticism of Darwin's work that has appeared in America. This book has won high praise and deserves it, for Morgan performed a distinct service in gathering together and presenting in good form all that could be said against Darwin and his theories. It is imposing in mass but hardly convincing in detail, giving an impression of painstaking special pleading rather than the judicial attitude which Darwin so successfully maintained throughout his work. Moreover, one has the feeling that the outcome is something of an anticlimax; because the author, if he gets anywhere, simply arrives in the camp of the mutationists, who, like their leader, De Vries, are mainly Darwinian in their belief now as they were then.

In 1903 mutations, so far as they were known, were relatively large jumps, while subsequent investigations have shown them to be entirely comparable, phenotypically, with the "individual variations" of Darwin, and they would have been so regarded by him. There was therefore a much greater apparent difference between fluctuations, or individual variations of Darwin, and the mutations of De Vries in 1903 than there is now; for these differences, so far as outwardly shown, (and these were all that Darwin did, or could deal with) have practically disappeared.

It can not be too strongly insisted upon that the great proportion of the mutants of the present time were included in the variations of Darwin.

At the conclusion of Morgan's work we find the following statement which is most significant from the standpoint of this paper: "Their (the Darwinian school's) opponents, on the other hand, have, I believe, gone too far when they state that the present condition of animals and plants can be explained without applying the test of survival, or, in a broad sense, the principle of selection among species." "I am not unappreciative of the great value of that part of Darwin's idea which claims that the *condition* of the organic world, as we find it, can not be accounted for entirely without applying the principle of selection in one form or another. This idea will remain, I think, a most important contribution to the theory of evolution." Now the *condition* of the organic world was just what Darwin was trying to explain.

I have dwelt at some length on Morgan's work because I suspect that he is largely responsible for the attitude of Burroughs. But in the quotation just given Morgan admits a belief that Darwin's idea of selection is a most important contribution to the theory of evolution. After a somewhat relentless attempt to tear down most of Darwin's work, he allows his particular thing, *selection*, to stand with his approval; reluctantly and with reservation, it is true; but still to stand. It does not appear, then, that even the most iconoclastic of Darwin's critics would go so far as to regard Darwin as completely "shorn."

IV

A majority of naturalists are at present much impressed by the mutation theory and, if not fully convinced, are quite willing to be convinced of its truth. But it is also true that most of them are both Darwinians and mutationists, a position in which they but follow the lead of the master mutationist, De Vries. A still larger number of biologists are willing to subscribe to a belief in the Mendelian theory; and here, too, there is no quarrel with the idea of Natural Selection. The law of Mendel has given us a new insight into the mechanism of the germ cells and of heredity where Darwin had no means of pursuing his researches. But these more recent investigations have revealed nothing antagonistic to an acceptance of Natural Selection, which can now be regarded as a selection of the fittest mutants. And here again we must insist that the mutants of De Vries should properly be regarded as included in the individual variations of Darwin, with the exception of the very pronounced mutations, such as those of the evening primrose, which would have been regarded as "saltatory evolution" by earlier naturalists.

Such authorities as Castle and Jennings believe in Mendelism, but not in mutations. These also are far from believing that natural selection has been discarded.

Another writer who doubtless contributed ammunition for Mr. Burroughs' assault on Natural Selection is Professor H. F. Osborn, one of America's leading paleontologists. He also, it seems to me, does not deal quite fairly with the author of the "Origin," for he says: "Chance is the very essence of the original Darwinian hypothesis of evolution." In view of Darwin's frank and explicit explanation of his use of the word "chance," it is inexact, to say the least, to insist on a literal use of the word which he expressly disclaims and emphatically disavows. I do not believe, moreover, that Professor Osborn would fully endorse the unqualified statement regarding the shorn condition of Darwin; for he distinctly admits the importance of Natural Selection in his book "The Origin and Evolution of Life." He says:

"Upon the resultant actions, reactions and interactions of potential and kinetic energy in each organism selection is constantly operating." Again he says, and still more explicitly: "Whenever such changes of proportion weigh in the struggle for existence they may be hastened or retarded by Natural Selection."

As a matter of fact it is very doubtful if any leading zoologist would at the present time be willing to make so positive a statement as has John Burroughs regarding the "shorn" condition of Darwin so far as Natural Selection is concerned. Practically all of them regard Natural Selection as an important contribution to our understanding of organic evolution.

V

Indeed I must confess to a degree of skepticism regarding the mental attitude of the distinguished author of "A Critical Glance into Darwin" himself, for there are some rather glaring inconsistencies in his paper. After declaring the shearing process complete he finds himself unable to get along without appropriating some of the shorn locks to his own use. For instance: "And though I believe that *the accumulation of variations is the key to new species*, this acumulation is not based upon outward utility but on innate tendency to development." The italics are mine. No one, so far as I know, has proposed any method by which an accumulation of variations can be brought about except by the action of Natural Selection.

Again, he says: "Natural Selection turns out to be of only secondary importance—Natural Selection gives speed where speed is the condition of safety, strength where strength is the condition, keenness and quickness of sense-perception where these are demanded." Such expressions seem to indicate that Natural Selection functions in some

rather important matters after all, indeed that it is of at least secondary importance.

After criticising Darwin for presenting an anthropocentric view of Nature, he naively remarks that it (Natural Selection) "is Nature's way of improving her stock." Just the picture that Darwin drew!

But, as it seems to me, he practically abandons his whole case when he says: "What I mean to say is that there must be some primordial tendency to development which Natural Selection is powerless to beget. It can not give the wings to the seed, or the spring, or the hook—*but it can perfect all those things. The fittest of its kind does stand the best chance to survive.*" Again, the italics are mine.

Now if there be any one thing about which all biologists and physicists are in thorough agreement, it is the belief in the uniformity and continuity of law in the universe. All believe that Nature's laws have worked in the past just as they are working now. In other words, if Natural Selection is at present engaged in perfecting animals and plants, she has been doing just that thing throughout the ages since life first appeared.

But we must remember that, although this perfecting is continuous, the standard of perfection is constantly changing. Perfection is always relative and never absolute. The first true winged bird, the Archeopteryx, was doubtless a very imperfect, even crude, creature according to our notions; but it was in its day an immense advance over its competitors and the best animated flying-machine of its time. It was practical, too. It did the business of flying better than any of its rivals. It was fittest to survive and it did survive, in its modern descendants, from far-away Jurassic time to this. And during all these countless centuries Nature has been at work "perfecting" this uncouth creature; keeping its descendants, or some of them, ahead of the game and thus making it perpetually the "fittest." And from this absurd creature have "radiated" as Osborn would say, various sorts of winged creatures each adapted to its manner of life or environment. These radiating lines ended in such marvels as the man-o'-war bird, that master acrobat of the air; and that dainty flying jewel, the humming-bird, poised apparently motionless in space; and, in short, all of the aerial artists that we call birds.

One can not help wondering just what Burroughs' position is when he says: "They (organic beings and structural changes) are adaptive from the first. They do not need Natural Selection to whip them into shape," and then declares: "Natural Selection perfects" these things. It seems that the same agency does not "whip into shape," but it does "perfect" things. This leaves us somewhat dazed.

VI

What appears to bother Mr. Burroughs most of all is the seeming lack of any directive force in the process of evolution. He balks, as countless others have, at the idea that "This is a world of chance into which Darwin delivers us," and adds: "What can the thoughtful mind make of it?" Darwin himself balked at it, but was honest enough to admit that he saw no way out of the dilemma, and others have had like experience. Some sort of orthogenesis has appealed to many; but the trouble is there is little to prove its existence, however devoutly we may desire such proof. Darwin was too intensely honest in his intellectual processes to incorporate any such idea without some sort of evidence that would appeal to him as likely to carry conviction; but he found none, nor has any one else, so far as I can see. Can we doubt that he would have gladly welcomed and used such proof, had it been forthcoming?

What is Burroughs' proposal by which he seeks to satisfy this longing for something better than a world of chance? He says: I am persuaded that there is something immanent in the universe, pervading every atom and molecule in it, that knows what it wants—a cosmic mind or intelligence."

This must be pantheism, if I understand it at all; but there is little that is soul-satisfying about pantheism, and a religion that does nothing for the soul is poor stuff. Moreover, such a belief has no scientific sanction.

The groping of the thoughtful mind for some directive force in evolution, whether in the form of an entelechy, orthogenesis, predetermination, Providence, or the "horse impulse" of Burroughs, has in it something of pathos and reminds one of the statue to "the Unknown God" at Athens; but thus far there has appeared no Paul to make him known unto us. I am sure that Darwin would have welcomed him, but would have demanded some rather convincing credentials. The God that Paul did propose to the Athenians answers, in the minds of Christian evolutionists (and there are many of us) to the God of the second moollah in Burroughs' article for "He is so wise that he makes all things make themselves."

But it is utterly unnecessary to inject pantheism or any other first cause into a discussion of Natural Selection. Darwin did not attempt anything of the sort and therein showed his wisdom. He found that there are three fundamental laws of living things, each so obviously true as to be axiomatic. Nor was it germain to his purpose to go back of these laws and discuss ultimate causes. He simply took the world as he found it and explained how diversity of species had been brought about. The three laws are:

1st. *The Law of Heredity*, which works to make the progeny resemble their parents. No one doubts the *fact* of heredity whatever its mechanism may be. Darwin did try to imagine a sufficient mechanism and called it "pangenesis." This theory has been largely discarded; but, whether true or false, it has little direct bearing on the theory of Natural Selection, nor could it invalidate that theory in the least. This law is the *conservative* factor.

2nd. *The Law of Individual Variation.* No one doubts that this is a fact, whether he calls the variations "individual variations," "fluctuating varieties" or "mutations." They are all simply variations in the Darwinian sense. Darwin did not explain the origin of variations, nor did he need to. I have little sympathy with those who exclaim that "Natural Selection originates nothing" because it does not tell the cause of variation. When we contemplate the bewildering complexity of the germplasm and the countless hereditary possibilities in each germ cell, the wonder is not that there is variation, but that it is kept within such reasonable limits. This law is the *qualitative* factor of organic evolution, furnishing differences from which selection may be made.

3rd. *The Law of Geometrical Ratio of Increase*, by which every species tends to over-populate the earth if its natural ratio is not checked. No one doubts that this is a fact. Indeed it seems to have furnished both Darwin and Wallace with the key with which they each solved the problem of the origin of species. This law is the *quantitative* factor, furnishing great numbers from which to select.

Having stated these laws and discussed them at length, Darwin set himself the task of ascertaining how they would act and interact in Nature, and found that the necessary result of such action and interaction would be a *selection* that would result in more and more perfect adaptations to environments and the elimination of the unfit, thus assuring the survival of the fittest or most perfectly adapted forms, and a general advance in complexity and specialization of species. He compared this selection to the artificial selection wrought by man in improving his domestic animals and called it "Natural Selection." It seems to me puerile to object to this comparison, for Darwin pointed out in the most painstaking way the essential differences between the two. They are comparable in a large and true sense.

That a selection of some sort could result from the combined working of these three laws can not be successfully denied, and it was Darwin who proved this to the world. And no name has been proposed for this selection that is more satisfactory to biologists as a whole than "Natural Selection."

The logic of this course of reasoning seems to me and to many of my zoological, botanical and geological colleagues, unanswerable.

I feel very sure that a large majority of those professionally engaged in the study of biological science would answer the question: "Is Darwin completely shorn of his selection doctrines?" by a decided negative. Some of his ideas have been trimmed to suit the prevailing mode of thought and more complete knowledge; but he is not "shorn."

In conclusion I must venture to express the hope that the honored friend of Nature, John Burroughs, will not allow an incursion into the field of tonsorial art to divert his attention from that exquisite art with which he has so often delighted an innumerable host of sincere admirers. There are controversial writers galore, and in every field of thought; but few indeed are they who do the work of observing and depicting the lives and habits of our familiar friends of orchard, field and forest with so keen an insight and so sure and felicitous a touch.

THE BOTANY OF THE NEW ENGLAND POETS

By DR. NEIL E. STEVENS

BUREAU OF PLANT INDUSTRY

MODERN New England poets are necessarily excluded from this discussion. What Amy Lowell writes may be poetry, but her references to plants are certainly not botany. Adequate and appropriate tribute[1] has moreover been paid to her masterly ignorance of things botanical as displayed in a *vers libre* effusion on sugar printed in *The Independent* (December 29, 1917) in which the familiar red table beet is forced to assume the chemical and horticultural rôle of the coarse, whitish, turnip-like root known commercially as the sugar beet. To quote in part:

> Wide plains,
> With little red balls hidden under them,
> Beets like a hidden pavement underneath the plains.
> A Roman floor forsooth!
> Do mosaics have any color to equal these?
> Red as the eyes of cats in firelight,
> As carbuncles under a lemon moon,
> As the sun swirling out of a foggy sky,
> Round as apples,
> Footed as tops,
> You spin yourself deep into the earth
> And swell and fatten,
> Sugar in a crimson coat,
> . . . . . .
> And there still the blood-skinned beets,
> Waiting to be crushed, pulped, and eaten,
> Thunder sugar—blood sugar—

In contrast, attention will be confined to the New England poets in the most limited sense, that is to the poets of that little group, the most distinguished in American letters, usually known as the Cambridge School, namely, Longfellow, Whittier, Holmes, and Lowell. It is perhaps more than a coincidence that these men were the contemporaries and most of them the friends of that master mind of New England botany, Asa Gray.[2] Indeed it was of Gray that Lowell wrote:

> Just Fate, prolong his life well-spent
> Whose indefatigable hours
> Have been as gaily innocent
> And fragrant as his flowers.[3]

[1]Broadhurst, Jean. Botanical Errors of some well-known writers. Torreya, p. 118, 1918.

[2]Gray 1810-1888. Whittier 1807-1893. Lowell 1819-1891. Longfellow 1807-1882. Holmes 1809-1894.

[3]The occasion was Professor Gray's seventy-fifth birthday. A silver memorial vase inscribed with the legend,

Bryant's birth and early life in the Berkshires place him among the New England poets though not in the Cambridge school, and his repute as a nature poet might indicate that he would be entitled to special botanical consideration. In only a few of his poems, however, does one find evidence of accurate botanical knowledge. "The Death of the Flowers," "The Old Man's Council," "The Fountain," and "To the Fringed Gentian," though, show his interest in and love for flowers.

Emerson, although not primarily a poet, belongs of course to the Cambridge school, and there is evidence of considerable botanical knowledge in his poems. In "Nature" for example he names over forty species of plants. If, however, Emerson had been a botanist, and he would have made a very good one, he would not have been a systematist. In 1858 few botanists even took the trouble to record the size of the trees they mentioned, as he does in "The Adirondacs"

> Our patron pine was fifteen feet in girth,
> The maple eight, beneath its shapely tower.

Nor is there to be found in all the good natured abuse showered upon the systematists by physiologists, ecologists and plant breeders during the last ten years anything more caustic, than Emerson's comment in "Blight"

> But these young scholars, who invade our hills,
> . . . . . .
> Love not the flower they pluck, and know it not,
> And all their botany is Latin names.

In Longfellow's poems there are to be found, of course, fine lines which express realization of the beauty of field and forest, and there is evidence of some botanical knowledge. No careful reader of his poetry will, however, maintain that Longfellow appreciated plants or plant life as he did the ocean, or even inland bodies of water. Some explanation of this may perhaps be found in the surroundings of his youth. Born within sight of the sea and his boyhood spent in a seaport town, it is natural that the ocean should have left an impression on his mind unequaled by other natural objects. Indeed in "My Lost Youth" the first five stanzas deal chiefly with the sea and the harbor including:

> I remember the black wharves and the slips,
> And the sea-tides tossing free;
> And the Spanish sailors with bearded lips,
> And the beauty and mystery of the ships,
> And the magic of the sea.

Whereas the only references to things botanical are to the trees that

---

"1810 November eighteenth 1885
Asa Gray
in token of the universal esteem of American botanists"
was presented to Professor Gray accompanied by the lines quoted above and by greetings from 180 American botanists. See Jordan, David Starr, Leading American Men of Science. New York. 1910. Asa Gray, Botanist; by John M. Coulter, p. 211-231.

line the streets, and to Deering's Woods, a grove in the outskirts of Portland.

Whittier, most of whose early life was spent on a farm, knew the names of a good many plants, but even when he mentions them it is usually in their relation to human life or as symbolic of human activity. On receiving a sprig of heather in blossom, he writes a poem for "Burns" and in writing of "The Sycamores" his thoughts are very little with the trees but chiefly of the Irishman, Hugh Tallant, who planted them. The trailing arbutus moves him to two poems; but one "The Mayflowers," deals chiefly with the Pilgrims, and a shorter one "The Trailing Arbutus" ends thus

> As, pausing, o'er the lonely flower I bent,
> I thought of lives thus lowly, clogged and pent,
> Which yet find room,
> Through care and cumber, coldness and decay,
> To lend a sweetness to the ungenial day,
> And make the sad earth happier for their bloom.

It is the labor of the farm, rather than the wild life of the farm, that appeals to Whittier (see his "Songs of Labor"); and one is led to the belief that a very real interest in the man who works with his hands, as well as his creed as a Friend, inspired him in his long fight against slavery.

Even as a boy, however, Whittier apparently showed no special interest in plants. In fact, it would seem that such biological inclinations as he possessed were zoological rather than botanical. If one examines "The Barefoot Boy's" stock of nature lore

> Knowledge never learned of schools,
> Of the wild bee's morning chase,
> Of the wild-flower's time and place,
> Flight of fowl and habitude
> Of the tenants of the wood;
> How the tortoise bears his shell,
> How the woodchuck digs his cell,
> And the ground-mole sinks his well;
> How the robin feeds her young,
> How the oriole's nest is hung;
> Where the whitest lilies blow,
> Where the freshest berries grow,
> Where the groundnut trails its vine,
> Where the wood-grape's clusters shine;
> Of the black wasp's cunning way,
> Mason of his walls of clay,
> And the architectural plans
> Of gray hornet artisans!—

he will come to the conclusion that this particular boy was much more interested in animals than in plants. For against a very creditable list of items of information regarding quadrupeds, birds, and insects are balanced only one generalization about flowers, and three plants regarded as sources of food. Indeed, for a poet, and a life long member of the Society of Friends, Whittier betrays an almost surprising

interest in gastronomic pleasures. He exclaims with many another New Englander (The Pumpkin)

> What moistens the lip and what brightens the eye?
> What calls back the past, like the rich Pumpkin pie?

Holmes' medical training gave him a knowledge of drug plants, which he uses to good advantage in a professional poem "Rip Van Winkle, M. D." Holmes seems to have had, moreover, a special interest in and affection for trees, references to them occur again and again in his poems. That this interest dates from his boyhood is evidenced by a poem "The Meeting of the Dryads" written after a general pruning of the trees around Harvard College, some time before he was sixteen. With real feeling the spirit of one of the mutilated trees speaks

> Go on, Fair Science; soon to thee
> Shall Nature yield her idle boast;
> Her vulgar fingers formed a tree,
> But thou hast trained it to a post.

and the poem concludes with the following inclusive curse upon the "tree surgeon" of Harvard

> A curse upon the wretch who dared
> To crop us with his felon saw!
> May every fruit his lip shall taste
> Lie like a bullet in his maw.
>
> . . . . . .
>
> May nightshade cluster round his path,
> And thistles shoot, and brambles cling;
> May blistering ivy scorch his veins,
> And dogwood burn, and nettles sting.
>
> On him may never shadow fall,
> When fever racks his throbbing brow,
> And his last shilling buy a rope
> To hang him on my highest bough!

Holmes most frequently uses botanical information in figures, and his botanical figures are usually apt. The rose, the lily and the peach have been so much used, and abused by poets in their attempts to do justice to feminine beauty that Holmes' courageous use (in The Autocrat at the Breakfast Table) of the more beautiful but less conspicuous cranberry blossom deserves special mention

> My lady's cheek can boast no more
> The cranberry white and pink it wore.

With a vigor equaled only by the preacher's "of making many books there is no end; and much study is a weariness of the flesh" he sums up the passion for publication, chiefly in botanical figures, in Cacoethes Scribendi

> If all the trees in all the woods were men;
> And each and every blade of grass a pen;
> If every leaf on every shrub and tree
> Turned to a sheet of foolscap; every sea

Were changed to ink, and all earth's living tribes
Had nothing else to do but act as scribes,
And for ten thousand ages, day and night,
The human race should write, and write, and write,
Till all the pens and paper were used up,
And the huge inkstand was an empty cup,
Still would the scribblers clustered round its brink
Call for more pens, more paper, and more ink.

The poet is honest, however, and owns that he is one of the scribblers, when in one of the "Poems of the Class of '29" he sums up the whole matter,

"Why won't he stop writing!" Humanity cries:
The answer is briefly, "He can't if he tries."

Perhaps his best figure, certainly his best known, is a botanical one, "The Last Leaf." It is a curious commentary on the failure of many people to observe the most common natural phenomena that in the latter editions of his poems, Holmes felt obliged to insert in the introduction to the poem bearing the above title an explanation of a relation which should have been obvious to every one who had seen oak trees in New England in early spring. His own fondness for so apt a figure is shown by its repeated use. In 1831 (The Last Leaf) he wrote

And if I should live to be
The last leaf upon the tree
In the spring,
Let them smile, as I do now,
At the old forsaken bough
Where I cling.

Thirty years later he uses the same figure (The Old Player 1861)

Yet there he stood,—the man of other days,
In the clear present's full, unsparing blaze,
As on the oak a faded leaf that clings
While a new April spreads its burnished wings.

And in 1889 in acknowledging the gift of a silver loving cup, he wrote (To The Eleven Ladies)

"For whom this gift?" For one who all too long
Clings to his bough among the groves of song;
Autumn's last leaf, that spreads its faded wing
To greet a second spring.

Lowell was deeply interested in the turbulent political events of his time and The Biglow Papers are the soul of his published work. However, his poems bear evidence of deep interest in and accurate knowledge of natural objects, particularly plants. He does not seem to overstate the case when in "To George William Curtis" he tells of his interest in out of door things and apologizes, almost, for his humanitarian interests:

Dear were my walks, too, gathering fragrant store
Of Mother Nature's simple-minded lore:
I learned all weather-signs of day or night;
No bird but I could name him by his flight,

No distant tree but by his shape was known,
  Or, near at hand, by leaf or bark alone.
This learning won by loving looks I hived
  As sweeter lore than all from books derived.
I know the charm of hillside, field and wood,
  Of lake and stream, and the sky's downy brood,
Of roads sequestered rimmed with sallow sod,
  But friends with hardhack, aster, golden-rod,[4]
Or succory keeping summer long its trust
  Of heaven-blue fleckless from the eddying dust:
These were my earliest friends, and latest too,
  Still unestranged, whatever fate may do.
For years I had these treasures, knew their worth
  Estate most real man can have on earth
I sank too deep in this soft-stuffed repose
  That hears but rumors of earth's wrongs and woes;
Too well these Capuas could my muscles waste,
  Not void of toils, but toils of choice and taste;
These still had kept me could I but have quelled
  The Puritan drop that in my veins rebelled.

But there were times when silent were my books
  As jailers are, and gave me sullen looks,
When verses palled, and even the woodland path,
  By innocent contrast, fed my heart with wrath,
And I must twist my little gift of words
  Into a scourge of rough and knotted cords
Unmusical, that whistle as they swing
  To leave on shameless backs their purple sting.

How slow Time comes! Gone, who so swift as he?
  Add but a year, 'tis half a century
Since the slave's stifled moaning broke my sleep,
  Heard 'gainst my will in that seclusion deep,
Haply heard louder for the silence there,
  And so my fancied safeguard made my snare.

Whatever Lowell's knowledge of botany he certainly understood and appreciated scientists and quite properly places Jonah among them. (The Biglow Papers)

> Men in general may be divided into the inquisitive and the communicative. To the first class belong Peeping Toms, eaves-droppers, navel-contemplating Brahmins, metaphysicians, travellers, Empedocleses, spies, the various societies for promoting Rhinothism, Columbuses, Yankees, discoverers, and men of science, who present themselves to the mind as so many marks of interrogation wandering up and down the world, or sitting in studies and laboratories. . . . . .
>
> To one or another of these species every human being may safely be referred. I think it beyond a peradventure that Jonah prosecuted some inquiries into the digestive apparatus of whales, and that Noah sealed up a letter in an empty bottle, that news in regard to him might not be wanting in case of the worst. They had else been super or subter human.

[4]For the guidance of those unfamilar with the common names there are included in several places lists of the plants mentioned with their Latin names as given in Gray's Manual, Seventh Edition. Hardhack, *Spiraea tomentosa* L.; Golden-rod, *Solidago sp.;* Succory, *Cichorium Intybus* L.

Horticulture

It is difficult to determine just what should be considered as significant horticultural information. Longfellow, for example, in "Catawba Wine" shows that he knew the names at least of several varieties of wine grapes, but such knowledge could of course be acquired, at least before July 1, 1919, without personal study of the vineyard. On the other hand, in "The Birds of Killingworth" he shows that he understands better than many of his successors the close relation between birds, and insects, and the relation of insects to the destruction of crops. Though the great destruction of crops brought about by the increased abundance of insects comes rather suddenly after the death of the birds.

Here should certainly be included, however, an observation made by Lowell in a note introductory to one of the Biglow Papers

> The two faculties of speech and of speech-making are wholly diverse in their natures. By the first we make ourselves intelligible, by the last unintelligible, to our fellows. It has not seldom occurred to me (noting how in our national legislature everything runs to talk, as lettuces, if the season or the soil be unpropitious, shoot up lankly to seed, instead of forming handsome heads) that Babel was the first Congress, the earliest mill erected for the manufacture of gabble.

The horticultural significance of this quotation lies of course in the parenthetical observation that under conditions unfavorable for vegetative growth, lettuce like many other vegetables runs to seed.

In these days when grades and standards for packing fruit, and the accurate labeling of foods and drugs are an important part of the activity of a Department of Agriculture it may be interesting to note some of the things Holmes considered "essential to a Millennium" (Latter-Day Warnings)

> When legislators keep the law,
> When banks dispense with bolts and locks—
> When berries — whortle, rasp, and straw—
> Grow bigger *downwards* through the box,
> . . . . . .
> When preachers tell us all they think,
> And party leaders all they mean,
> When what we pay for, that we drink,
> From real grape and coffee-bean,
> . . . . . .
> *Till* then let Cumming blaze away,
> And Miller's saints blow up the globe;
> But when you see that blessed day,
> *Then* order your ascension robe!

We are indebted to Whittier's interest in all that pertained to advocates of the abolition of negro slavery, for a poetic record of seed and plant introduction carried on previous to 1700 by Francis Daniel Pastorius,[5] one of the earliest German immigrants to Pennsylvania. According to Whittier a correspondence and exchange of plants was

carried on between Pastorius and his friends and teachers in Germany (The Pennsylvania Pilgrim):

> And thus the Old and New World reached their hands
> Across the water, and the friendly lands
> Talked with each other from their severed strands.
>
> Pastorius answered all: while seed and root
> Sent from his new home grew to flower and fruit
> Along the Rhine and at the Spessart's foot;
>
> And, in return, the flowers his boyhood knew
> Smiled at his door, the same in form and hue,
> And on his vines the Rhenish clusters grew.

Mycology and Phytopathology

In view of the scant consideration given fungi by New England botanists before Farlow, it may be worth mentioning that at least two of the New England poets knew that fungi existed. Holmes mentions fungi frequently. One of his early poems is entitled "The Toadstool" and Emerson refers in "Nature" to

> Where the fungus broad and red
> Lifts its head,
> Like poisoned loaf of elfin bread.

Moreover, from two chance references I am convinced that Holmes knew, what some botanists had yet to learn, that a lichen is really a fungus. In Astraea he contrasts the "arctic fungus" and the desert palm; and in "A modest request" he refers to

> —the rock where nature flings
> Her arctic lichen, last of living things;

Mention of plant diseases are confined chiefly to generalized references to mold, mildew and blight, but in the Biglow Papers there is evidence that Lowell knew Peach Yellows and some of its symptoms.

> Take them editors thet's crowin'
> Like a cockerel three months old,—
> Don't ketch any on 'em goin',
> Tho they be so blasted bold;
> Aint they a prime lot o' fellers?
> 'Fore they think on't guess they'l sprout
> (Like a peach thet's got the yellers)
> With the meanness bustin' out.

Ecology

In these days when ecology has a society, a special journal, and a special language it would be a presumptuous botanist indeed, who, not one of the elect, should attempt to decide what is ecology and what

[5]Jellett, Edwin C., Germantown Gardens and Gardeners, 1914. On pages 5—16 of this work is given an account of Francis Daniel Pastorius, the founder of Germantown, which indicates that Whittier's account is substantially correct.

is not, and it would be even more presumptuous to suppose that in poets of the last generation one would find today's ecology. But in the old fashioned sense of plant relations, the relations of plants to each other and to their environment we find, in Lowell at least, evidence of real comprehension. Any one who has carefully watched the almost unbelievably rapid development of vegetation during the spring of northern New England, and contrasted it with the abject manner in which spring approaches in southern California will appreciate the feeling and the keen observation evidenced in Lowell's description of spring in the Biglow Papers. Beginning on May Day

I, country-born an' bred, know where to find
Some blooms thet make the season suit the mind,
An' seem to metch the doubtin' bluebird's notes,—
Half-ventrin' liverworts[6] in furry coats,
Bloodroots, whose rolled-up leaves ef you oncurl,
Each on' em's cradle to a baby-pearl,—
But these are jes' Spring's pickets; sure ez sin,
The rebble frosts 'll try to drive 'em in;
For half our May's so awfully like May n't,
't would rile a Shaker or an evrige saint;
Though I own up I like our back'ard springs
Thet kind o' haggle with their greens an' things,
An' when you 'most give up, 'uthout more words
Toss the fields full o' blossoms, leaves an' birds:
. . . . . .

'fore long the trees begin to show belief,
The maple crimsons to a coral-reef,
Then saffern swarms swing off from all the willers
So plump they look like yaller caterpillars,
Then gray hossches' nuts leetle hands unfold
Softer'n a baby's be at three days old:
. . . . . .

Then seems to come a hitch,—things lag behind,
Till some fine mornin' Spring makes up her mind,
An' ez, when snow-swelled rivers cresh their dams
Heaped-up with ice thet dovetails in an' jams,
A leak comes spirtin' through some pin-hole cleft,
Grows stronger, fercer, tears out right an' left,
Then all the waters bow themselves an' come,
Suddin, in one gret slope o' shedderin' foam,
Jes' so our Spring gits everythin' in tune
An' gives one leap from Aperl into June:
Then all comes crowdin' in; afore you think,
Young oak-leaves mist the side-hill woods with pink;
The catbird in the laylock-bush is loud;
The orchards turn to heaps o' rosy cloud;
Red-cedars blossom tu, though few folks know it,
An' look all dipt in sunshine like a poet;

Not only is the succession of spring flowers correctly observed, and appropriate emphasis given to the rapidity of the development of the vegetation during the latter part of May, but the above quoted poem

[6]Liverwort, *Hepatica triloba* Chaix; Bloodroot, *Sanguinaria canadensis* L.; Maple, Acer, (probably *rubrum* L.); Willers (willow), *Salix sp.;* Hossches' nuts, (Horse-chestnut), *Aesculus Hippocastanum* L.; Oak, *Quercus sp.;* Laylock (lilac), *Syringa vulgaris* L.; Red-cedar, *Juniperus virginiana* L.

is the only record the writer has found of the fact that in New England at least the red-cedar blossoms at the same time as the cultivated apple.

Holmes expressed the idea of the rapid climatic change at this season with equal vigor in an early poem entitled "The Hot Season":

> The folks, that on the first of May
> Wore winter coats and hose,
> Began to say, the first of June,
> "Good Lord! how hot it grows!"

So far as vegetation is concerned, however, Holmes' idea of spring includes, as becomes a townsman, chiefly cultivated plants. In three poems (Poetry, Spring, and Spring Has Come) in which the season is described he mentions the snowdrop, the hyacinth, tulip, crocus, iris, narcissus, and peony; but among wild flowers only the violet.

Not less accurate, nor less vivid than his description of spring is Lowell's record of autumn foliage in An Indian Summer Reverie. During the four years just passed the writer has spent the fall months in eastern Massachusetts and has been unable to note any error either of time, color, or habit in the various species mentioned in the following quotation:

> What visionary tints the year puts on,
> When falling leaves falter through motionless air
> Or numbly cling and shiver to be gone!
>
> . . . . . .
>
> The sobered robin, hunger-silent now,
> Seeks cedar-berries[7] blue, his autumn cheer;
> The chipmunk, on the shingly shagbark's bough,
>
> . . . . . .
>
> O'er yon bare knoll the pointed cedar shadows
> Drowse on the crisp gray moss; the ploughman's call
> Creeps faint as smoke from black, fresh-furrowed meadows;
> The single crow a single caw lets fall;
> And all around me every bush and tree
> Say Autumn's here, and Winter soon will be,
> Who snows his soft, white sleep and silence over all.
>
> The birch, most shy and ladylike of trees,
> Her poverty, as best she may, retrieves,
> And hints at her foregone gentilities
> With some saved relics of her wealth of leaves;
> The swamp-oak, with his royal purple on,
> Glares red as blood across the sinking sun,
> As one who proudlier to a falling fortune cleaves.
>
> He looks a sachem, in red blanket wrapt,
> Who, 'mid some council of the sad-garbed whites,
> Erect and stern, in his own memories lapt,
> With distant eye broods over other sights,
> Sees the hushed wood the city's flare replace,
> The wounded turf heal o'er the railway's trace,
> And roams the savage Past of his undwindled rights.

[7]Cedar, *Juniperus virginiana* L.; Shag-bark *Carya ovata* (Mill.) K. Koch; Birch, Betula (probably here *populifolia Marsh.*); Swamp-oak, *Quercus palustris* Muench.; Red-oak, *Quercus rubra* L.; Chestnut, *Castanea dentata* (Marsh) Borkh.; Ash, *Fraxinus americana* L.; Maple (swamp), *Acer rubrum* L.; Scrub-oak, *Quercus ilicifolia Warg.;* Black-alder, *Ilex verticillata* (L.) Gray; Woodbine, *Psedera quinquefolia* (L.) Greene; Elm, *Ulmus americana* L.; Ivy (poison?), *Rhus Toxicodendron* L.

The red-oak, softer-grained, yields all for lost,
And, with his crumpled foliage stiff and dry,
After the first betrayal of the frost,
Rebuffs the kiss of the relenting sky;
The chestnuts, lavish of their long-hid gold,
To the faint Summer, beggared now and old,
Pour back the sunshine hoarded 'neath her favoring eye.

The ash her purple drops forgivingly
And sadly, breaking not the general hush;
The maple-swamps glow like a sunset sea,
Each leaf a ripple with its separate flush;
All round the wood's edge creeps the skirting blaze
Of bushes low, as when, on cloudy days,
Ere the rain falls, the cautious farmer burns his brush.

O'er yon low wall, which guards one unkempt zone,
Where vines, and weeds, and scrub-oaks intertwine
Safe from the plough, whose rough, discordant stone
Is massed to one soft gray by lichens fine,
The tangled blackberry, crossed and recrossed, weaves
A prickly network of ensanguined leaves;
Hard by, with coral beads, the prim black-alders shine,

Pillaring with flame this crumbling boundary,
Whose loose blocks topple 'neath the ploughboy's foot,
Who, with each sense shut fast except the eye,
Creeps close and scares the jay he hoped to shoot,
The woodbine up the elm's straight stem aspires,
Coiling it, harmless, with autumnal fires;
In the ivy's paler blaze the martyr oak stands mute.

The ecology of today includes, I believe, both animals and plants in the same study. Lowell's description of a New England pool is then both up to date and accurate. (Festina Lente):

Once on a time there was a pool
Fringed all about with flag-leaves[8] cool
And spotted with cow-lilies garnish,
Of frogs and pouts the ancient parish.
Alders the creaking redwings sink on,
Tussocks that house blithe Bob o' Lincoln
Hedged round the unassailed seclusion,
Where muskrats piled their cells Carthusian;
And many a moss-embroidered log,
The watering-place of summer frog,
Slept and decayed with patient skill,
As watering-places sometimes will.

No phenomenon more often interests the botanist in rural New York and New England than the persistence of certain plants about the sites of abandoned homesteads. Holmes records the plants he observed about one such in "The Exile's Secret."

Who sees unmoved, a ruin at his feet,
The lowliest home where human hearts have beat?
Its hearthstone, shaded with the bistre stain
A century's showery torrents wash in vain;
Its starving orchard, where the thistle blows
And mossy trunks still mark the broken rows;

[8]Flag (Sweet), *Acorus Calamus* L.; Cow-lilies, *Nymphaea advena*, Ait.; Alder, *Alnus rugosa* (Du Roc) Spreng.

Its chimney-loving poplar, oftenest seen
Next an old roof, or where a roof has been;
Its knot-grass,[9] plantain, —all the social weeds,
Man's mute companions, following where he leads;
. . . . . .
Its woodbine, creeping where it used to climb;
Its roses, breathing of the olden time;

Whether the original inhabitant of this "Island Ruin" was so unconventional as not to have had a lilac, or the lilac was unable to persist in so exposed a place cannot, of course, be determined. But it is hardly to be supposed that Holmes could have missed what is usually the most conspicuous of the relics. Whittier reports it in a similar situation (The Homestead)

A lilac spray, once blossom clad,
Sways bare before the empty rooms;
Beside the roofless porch a sad
Pathetic red rose blooms.

Wood Technology

Holmes is probably supreme in this field although the landlord in Longfellow's "Tales of A Wayside Inn" knows the fuel value of Oak, Maple and Apple, and in "Hiawatha" the canoe is built from appropriate and serviceable materials. In "Poetry" Holmes shows that he knows some of the penalties for using green wood for construction purposes, and it is no poetic license, but accurate observation of natural phenomena that records the sprouting from the unhewn timbers on the shady, and moister, side.

Scarce steal the winds, that sweep his woodland tracts,
The larch's[10] perfume from the settler's axe,
Ere, like a vision of the morning air,
His slight framed steeple marks the house of prayer;
Its planks all reeking and its paint undried,
Its rafters sprouting on the shady side,

Likewise the Deacon in building his now famous "One-hoss shay" uses, according to Holmes' record, the best of materials and judgment. He

inquired of the village folk[11]
Where he could find the strongest oak,
That couldn't be split nor bent nor broke,—
That was for spokes and floor and sills;
He sent for lancewood to make the thills;
. . . . . .
The crossbars were ash, from the straightest trees,
The panels of white-wood, that cuts like cheese,
But lasts like iron for things like these;
The hubs of logs from the "Settler's ellum,"—
Last of its timber,—they couldn't sell 'em,
Never an axe had seen their chips,
And the wedges flew from between their lips,
Their blunt ends frizzled like celery-tips;

[9]Knot-grass (Knotweed), *Polygonum sp.*; Plantain, *Plantago major*, L.
[10]Larch, *Larix laricina* (Du Roi) Koch.
[11]The only mention of the botanical significance of this poem which the

Careful review of the woods used, in the light of the information now available, leads one to believe that when combined with "Steel of the finest, bright and blue" they would go far to make good the Deacon's vow that

> He would build one shay to beat the taown
> 'N' the keounty 'n' all the kentry raoun';
> It should be so built that it could n' break daown;
> "Fur," said the Deacon, "'t's mighty plain
> That the weakes' place mus' stand the strain;
> 'N' the way t' fix it, uz I maintain,
> Is only jest
> T' make that place uz strong uz the rest."

Though only by the most extreme care in construction, and a timber inspection even more rigid than that used in the construction of airplanes could one guarantee a result like that reported by Holmes, for

> Little of all we value here
> Wakes on the morn of its hundredth year
> Without both feeling and looking queer,
> In fact there is nothing that keeps its youth,
> So far as I know, but a tree and truth.

It is, then not surprising that at this age,

> There are traces of age in the one-hoss shay,
> A general flavor of mild decay,
> But nothing local, as one may say.

or that the dramatic conclusion of the experiment occurs on this day.

> All at once the horse stood still,
> Close by the meet'n'-house on the hill,
> First a shiver, and then a thrill,
> Then something decidedly like a spill,—
> And the parson was sitting upon a rock,
> At half past nine by the meet'n'-house clock,—
> Just the hour of the Earthquake shock!
> What do you think the parson found,
> When he got up and stared around?
> The poor old chaise in a heap or mound,
> As if it had been to the mill and ground!
> You see, of course, if you're not a dunce,
> How it went to pieces all at once,—
> All at once, and nothing first,—
> Just as bubbles do when they burst.

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writer has ever seen in print is the "Young People's Arbor" conducted by James Lawler, Canadian Forestry Journal 11:201-202 (1915).

Oak undoubtedly for this use, *Quercus alba* L.

Lancewood, The West Indian *Oxandra virgata*, ("He sent for lancewood.")

Ash, *Fraxinus americana* L.; White-wood, *Liriodendron Tulipifera* L.; Elm, *Ulmus americana* L.

ANTHONY VAN LEEUWENHOEK THE FIRST BACTERIOLOGIST

By DAVID FRASER HARRIS, M. D., B. Sc. (Lond.), D. Sc., F. R. S. E., F. R. Sc.,

PROFESSOR OF PHYSIOLOGY AND HISTOLOGY, DALHOUSIE UNIVERSITY, HALIFAX, N. S.

THERE were two great workers with the microscope in the latter half of the seventeenth century—the Italian, Malpighi, and the Dutchman, Leeuwenhoek. Of these the former was in his own time and still is in our day by far the better known man.

Malpighi was the father of all such as work with the microscope; the creator of histology everywhere, not merely of Italian, the father of both the histologies, vegetable and animal. Much of his life was spent in Bologna, the old-world, arcaded, sun-bathed city, the seat of a university founded more than 800 years ago.

But very different was the place to which my wife and I made a pilgrimage in the Christmas holidays of 1907, the unfrequented little town of Delft in Holland, if known in more than name to the great mass of my countrymen, known as the seat of the manufacture of a certain kind of pottery called "Delf."

Though the place is by no means devoid of interest, all its other interests are for biologists thrown into the shade by the recollection that this was the scene of Leeuwenhoek's life-long labors with the microscope, the place where the first bacteriologist was born, lived, worked, died and was buried. The town is on the line of the canal between Rotterdam and the Hague. Modern it certainly is not, for it was founded at the end of the eleventh century by Duke Geoffrey of Lorraine. In the palace or Prisenhof, William of Orange was assassinated in 1584; in the old church or Oude Kerk as old as the town itself, two of Holland's naval heroes, Van Tromp and Piet Hein, lie buried.

Its New Church, founded in 1381, is the last resting place of a most important person in the history of European theology, Hugo Grotius (Groot). Here, too, is the family tomb of the Princes of Orange. In 1654 there occurred a disastrous gunpowder explosion in which upwards of 1,200 inhabitants perished.

Delft is best known for its pottery, but this industry is almost extinct, the present manufactures being carpet-weaving, distilling and dyeing; there is a polytechnic school, a school of military engineering,

a school for the education of colonial civil servants, a school of design, a theater, and a lunatic asylum. The town which is built in the form of a square is rather gloomy, and its narrow streets, flanked by high houses, have stagnant little canals in the middle of them.

Such was the place at which we alighted from the Rotterdam train in the grey light of a January afternoon. Our fellow passengers stared as we left the carriage; they could understand our going on to the Hague, but what was there to detain an Englishman at Delft at that time of year?

Only interest in the history of biology could make one search out the ponderous Oude Kerk to view the tomb of a man whose very name is an unknown sound to all save a few. We knew, however, that in that ungainly old church towering over the crowded houses lay buried an indefatigable worker in Nature's invisible world, one whose greatest honor in life was, as it is the boast in the inscription on his tomb, to have been enrolled a Fellow of the Royal Society of London, or "of England," as it puts it.

The F. R. S. told Leeuwenhoek in his lifetime that he had not lived in vain. We know from the good work he did that, whether he had been made an F. R. S. or not, he belonged to the innermost circle of the scientific immortals. This F. R. S. was, perhaps, the one great event in a life devoid of incident, at any rate empty of anything like the incidents that filled Malpighi's.

Malpighi, for whom a chair was created by a Grand Duke and he a Prince of the Medici, who was invited to teach in the University of Messina, and then entreated to leave it for the chief professorship of medicine at Italy's premier University of Bologna, who was constrained to end his days in the elegant sinecure of physician to the visible head of Christendom, and who was an F. R. S. to boot—even his life has been described as devoid of incident! What, then, must be said of Leeuwenhoek's? Leeuwenhoek probably never left Holland, it is doubtful if he ever quitted Delft. That Leeuwenhoek was a person of reliable character may be incidentally gathered from his having been made curator or trustee for the widow of Jan van der Meer the painter who was born at Delft in 1632 and died in 1675.

Malpighi was a hereditary landowner, Leeuwenhoek began life as assistant or clerk to a linendraper. He worked all his days in the little town where he was born, and seemed content to know that not only his countrymen, but those generous men of science in the greatest city of the world had recognized his worth. Science had equal need of the landowner and the linendraper. According to some accounts Leeuwenhoek was a man of independent means which he had inherited from relatives who were brewers, but he seems for the greater part of his life to have held the post of beadle (bedellus) to some public body

in Delft. The document describing his duties which were light and his emoluments which were lighter is still extant.

We were not long in finding the Oude Kerk, nor the venerable guide inside it. He was dressed entirely in black, and wore a black skull cap. He immediately directed us to the tomb of van Tromp; of course, that was what the Englishman had come to see at such a time of year. Casting a passing glance at the elaborate marble sculptures on the gilded monument of the hero, I pushed on saying, "Any light is good enough to see van Tromp's tomb by, let me see Leeuwenhoek's before the dark." To the latter, accordingly, the reverend guide led us, correcting as we went along my pronunciation of the name. At last, then, we stood before the only monument besides his discoveries that keeps the name of the great Dutchman alive on earth; and it was erected, we found, by "his sorrowing daughter," not by any admiring strangers whatsoever. As a monument it is plain and even severe; while its setting in that huge, cold, white-washed, northern church is very cheerless. Not that it was exactly neglected; some hand had placed a wreath upon it not many weeks before. The long inscription is carved on a high, narrow slab of very dark stone—marble or granite —over which is placed a portrait medallion of Leeuwenhoek that thus stands out effectively; it represents the left profile under a full wig. Below the slab is an ornamental pediment of dark stone with another inscription, and above this a marble skull is placed which appears curiously out of place, but is evidently the result of the gruesome conventional taste in mortuary lapidary art prevailing at that time. Perched on the high slab is a marble urn surmounted by a gilded torch; on the white wall behind this a single arch or vaulting springs from two very unpleasing pilasters. From the summit of the arch a female face in white marble looks down on the tomb which is surrounded by a peculiarly ugly black iron railing. The inscription is cut in clear lettering. The sense of the Latin is: "Erected to the pious and everlasting memory of Anthony van Leeuwenhoek, member of the Royal Society of England, who, by the help of microscopes which he had himself made, scrutinized the most hidden things in Nature, and thus by assiduous study learned the secrets of the physical world. These wonders he described in his native language for the instruction and admiration of the whole world. Born at Delft, October 24, 1632, died there August 26, 1723." Another inscription states that "this monument is erected to her very dear father by his sorrowing daughter, Maria a Leeuwenhoek." Her own tomb is marked by a deeply incised stone in the pavement to the left of her father's monument. These deeply cut stones in high relief are a feature of the floors of Dutch churches; they make walking over them difficult and even hazardous to the unobservant.

In the printed account of the monuments in the church we were informed that van Leeuwenhoek's friend the Dutch poet, H. C. Poot, who, in some respects, is to be compared with the famous Scottish bard, Robert Burns, honored his memory with the following epitaph which is carved in the freestone block at the entrance to the grave: "Here lies Anthony van Leeuwenhoek, oldest member of the Royal Society of London; born in the town of Delft on 24th October, 1632, and died on the 26th of August, 1723, at the age of 90 years, 10 months, and 2 days. If, O wanderer, respect for great age and wonderful gifts is universal, then guide thy steps with reverence here, for here hoary science in Leeuwenhoek lies buried."

Once more one could not help comparing the tombs of the two great contemporary microscopists of the seventeenth century. How different their last resting places; here an air of chillness, cheerlessness and massiveness pervades the place, so different from the warmth, the color, the brightness and the tinsel of the southern church of St. Gregory.

Of course we returned to pay our respects to the great van Tromp, for we would not on any account have hurt the feelings of our grave and courteous cicerone who sold us a printed account of the tombs in his church and a faded, amateur photograph of Leeuwenhoek's monument. There being nothing else to detain us, we stepped out into the dark of the winter evening.

Here, then, within a few hundred yards of one another lie three of Holland's great men—van Tromp, Hugo Grotius and van Leeuwenhoek; a hero of Holland's naval wars, a hero in Holland's religious struggles, and a hero in the peaceful, silent conquests of science in the world of the "infinitely little." But whose work remains? What have we of van Tromp's today? We have his memory in naval history, and how it was his guns that made London quake and that turned old Albermarle down to the mouth of the Thames to reply to them; we have this gorgeous mausoleum in Delft, and we have the popular song beginning—

> Van Tromp was an admiral brave and bold,
> And he walked beside the sea.

but it would be difficult to name much else.

And what remains of Grotius, in his lifetime the world-read Arminian theologian? Under the dust of our libraries rest his treatises on Divinity, International Law, Political Liberty and Dutch History. And of Leeuwenhoek?—the whole science of bacteriology, Leeuwenhoek was the first human being to see a protozoan, a bacterium and a micrococcus, and those he first described were scraped from his own teeth. It was to the Royal Society of London that these discoveries fraught with such consequences to the science of medicine were an-

nounced. The Royal Society has been at the birth of a good deal that has moulded humanity.

The passage first announcing the discovery of bacteria is in a paper, entitled "Animals in the scurf of teeth," dated September 17, 1683, published in Vol. XIV of the Phil. Trans. (1684), in which we read: "Besides these animals there were a great quantity of streaks or threads of different lengths, but like thickness lying confusedly together, some bent and some straight. These had no motion or life in them" (evidently Leptothrix buccalis). He had just described some motile protistae which he had killed by rinsing his mouth out with vinegar.

Of course Leeuwenhoek was not what we now mean by a bacteriologist; bacteriology began with Pasteur; but scientific microscopy owes a great part of its existence to the Dutchman who ground his own lenses and left a cabinet of twenty-six of them to the Royal Society which has lost them.

Baker in his treatise on the microscope says that Leeuwenhoek's lenses were not spheres or globules, but double convex lenses, with magnifying powers ranging from 40-160 diameters.

According to Professor Stirling, the microscopes used were still of the type of "simple" microscope, high as the magnifying powers were which were reached with them. "Each consisted of a small biconvex lens placed in a socket between two plates of brass which were riveted together and pierced with a small hole opposite the lens. The object to be examined was fixed at a convenient distance and its focal distance adjusted by screws." As no eye-piece was used, the microscope was technically a "simple" microscope.

"His capital discovery was undoubtedly that of the capillary circulation of the blood first announced in 1690," says the late W. B. Carpenter in the Encyclopaedia Britannica. Now, if this means that Leeuwenhoek's discovery of the blood moving in capillaries was the chief thing he saw under the microscope, it is possibly true from a physiological, although not from a morphological, point of view. Leeuwenhoek was not, however, the first to see the flowing blood in capillaries, for in 1660 Malpighi in Bologna had seen the blood flowing in those of the frog's lung. Leeuwenhoek thus discovered the capillary circulation for himself though not for the scientific world which had been informed of it through a letter from Malpighi at Bologna to Alphonso Borelli at Pisa. However the exact date of Leeuwenhoek's seeing it for the first time is 1688, not 1690, or twenty-eight years after Malpighi's discovery.

There is before me an English translation of Leeuwenhoek's classic: "The true circulation of the blood, and also that the arteries and veins are continued blood-vessels clearly set forth, described in a

letter written to the Royal Society of London by Antoni van Leeuwenhoek, member of this same Society." The letter is dated September 7, 1688; and as he says that the observations that led to it began with his finding frogs' eggs in May of that year, there is no doubt that it was between May and September, 1688, that he saw living capillaries in the tail of the tadpole, the tail of the fish (stickleback), and in the web of the frog's foot. He verified and extended Malpighi's discovery without apparently knowing anything about Malpighi's work; he does not mention him in this paper; possibly he had not seen any of his writings or even heard of him. Delft was as regards science in a backwater, and for long after 1688 scientific news did not travel very fast.

True, Delft is not far from the university town of Leyden, but it is doubtful if there was one microscope in the whole university at that time. There must have been one in the town, for a student, Ludwig Hamm, discovered spermatozoa in 1674 by the aid of one, a discovery which Leeuwenhoek also made independently of Hamm. But by 1688 the time had not come for the introduction of physical methods into the study of medicine in the class rooms of Leyden, that was left for Boerhaave, whose discourse on "The Use of the Mechanical Method in Medicine" was given at Leyden in 1703 as the inaugural address of the third year of his professorship.

Leeuwenhoek does, however, allude to his countryman, Swammerdam, whom he quotes on the morphology of tadpoles. In this passage we find that Swammerdam mentions the "incomparable Dr. Harvey's anatomical descriptions."

The late Sir Michael Foster, in his truly admirable lectures on "The History of Physiology in the 16th, 17th and 18th centuries," gives the date of Leeuwenhoek's seeing capillaries as "1668" (p. 98); the second "6" may be a misprint for an "8."

Although Delft was not a university town, and Leeuwenhoek not a university man and not in that way brought officially into contact with men of science, yet he happened to have as scientific confrère a young physician of considerable anatomical attainments, Regner de Graaf. This was no other than the discoverer of the ovarian follicles since known as "Graafian," and the author of a treatise on the pancreatic juice. In 1673, the year in which de Graaf died, he introduced Leeuwenhoek to the notice of the Royal Society to the fellowship of which he was elected in 1680. Leeuwenhoek contributed papers to its Transactions which may be seen in Numbers 94 to 380, but his first letter to the Secretary Oldenburg was as early as 1674. He also sent twenty-seven scientific letters to the French Academy.

As regards the red blood corpuscles or erythrocytes, Leeuwenhoek, although not the first to see them, was certainly the first, recognizing

what he saw, to describe them correctly as circular in man and oval in the vertebrata below man. As early as 1674 he gave an account of these bodies in the Philosophical Transactions. But Jan Swammerdam (1637-1680) in 1658 had seen erythrocytes in frog's blood and correctly attributed the redness of the blood to these and not to the plasma; this observation was not published until 1738 by Boerhaave in the "Biblia Naturae," a posthumous edition of the works of Swammerdam. Malpighi in 1665 published an account of adipose tissue as he thought he saw it in the omentum. He described what he took to be fat-cells in the capillaries of the membrane, they were really erythrocytes. Thus as Swammerdam's observations were not published, and Malpighi's, although published, erroneous even if accessible to Leeuwenhoek, we may in a certain sense give Leeuwenhoek the credit of the priority of discovery in this matter.

It was, of course, by the presence of these cells in the blood that Leeuwenhoek could see that it moved. It is interesting to note that he is entirely Harveian in his conception of the circulation. He writes: "I could distinctly perceive the whole circuit of the blood in its passage to the extremities of the vessels and in its return to the heart." Quite recently there has been published a reprint of Leeuwenhoek's classical paper; it is one of several such in a work entitled "Opuscula selecta Neerlandicorum de arte medica." It has an English translation on the opposite side of each page and it is illustrated by facsimiles of the original illustrations of the blood-vessels in the tadpole and stickleback. Along with it goes what seems an excellent portrait of Leeuwenhoek representing him in a full wig seated at a table, lens in hand.

As to the circulation of the blood, of course there were unbelievers; each new thing has been doubted. Perhaps one of the most curious paragraphs of Leeuwenhoek's paper on the circulation is the last in which he enumerates for the benefit of the Royal Society three respectable men who had witnessed the capillary circulation. He says that people had remarked: "Must we believe it because Leeuwenhoek tells us?" and so he produces eye-witnesses. "But now that I hear that more credit will be given to my words when I mention the names of those who have partly seen the aforesaid circulation of the blood about which I write to your honorable Society, and which I have discovered, I have no objection to mentioning instead of many, such as I trust will deserve most belief, as, for example, Mr. Cornelius Gravesande, M. D., and ordinary professor of anatomy and surgery, and also councillor and late sheriff of this town; Mr. Cornelius Valensis, also councillor and late sheriff; Mr. Antoni Heinsius, LL. D., councillor and 'pensionaris' of this town, late Envoy Extraordinary to His Majesty the King of France, and not long ago Ambassador of this State to the Court of His Royal Majesty of England. To these gentle-

men, to whom I usually communicate many of my discoveries, I have also shown the true circulation of the blood as distinctly as we see the current of the water in a running river with our naked eyes." From this we see that Leeuwenhoek entirely believed himself to be the discoverer of the capillary circulation. Perhaps the best known man who ever looked through a lens of Leeuwenhoek was the Czar, Peter the Great. He passed through Delft in 1698 and asked the man of science to visit him and bring his microscope with him. It is said that the Czar was much impressed by the spectacle of the circulation in the tail of a small eel. The East India Company from time to time supplied Leeuwenhoek with specimens.

In human histology Leeuwenhoek described the dentinal tubules, the fibers of the crystalline lens, the medulla of the hair-shaft, the epidermis, the spermatozoa, and a very great deal else; he also first discovered the striation in skeletal muscle. Leeuwenhoek was undoubtedly the first to give anything like a systematic account of the finer structure of certain insects, he described the scales on the wings, the tracheae, and the claws and appendages of spiders. He noted that the eyes of insects as well as of crustacea are facetted or compound. Besides giving a full account of the anatomy of the flea (Pulex irritans) he studied its development showing that it did not arise from sand, dust or dirt as was variously held in his day, but that it arose from an egg like every other creature he knew and passed through a metamorphosis. He remarks that "This despised creature is endowed with as great perfection in its kind as any large animal."

He observed that the pupa of the flea was attacked and fed upon by a minute parasite. Jonathan Swift having got hold of this fact made use of it in the following clever but now hackneyed lines:

> The vermin only tease and pinch
> Their foes superior by an inch.
> So Naturalists observe, a flea
> Has smaller fleas that on him prey,
> And these have smaller still to bite 'em
> And so proceed ad infinitum:
> Thus every poet in his kind
> Is bit by him that comes behind.

It was Leeuwenhoek who first gave a satisfactory account of the Aphides and the stages of their development. He showed that what had been called "ants' eggs" were the pupae of the Aphides, that the real eggs of Aphides were far smaller and underwent development into larvae. W. B. Carpenter said that Leeuwenhoek was the first to see a Foraminifer, when in the stomach of a shrimp he found what is now called Nonionina. He gave excellent figures of Balanus. He dis-

"Poetry, a Rhapsody," 1733, Vol. XIV., p. 311, Works of Swift, Scott's Edition.

covered Rotifers and gave an account of their surviving desiccation, a condition now known as latent life.

Leeuwenhoek by no means neglected vegetable histology, for he investigated the structure of oak, elm, beech, willow, and fir, noting the "pitting" in conifers and giving an account of medullary rays. He has left accurate descriptions of the embryo in germination and its relation to the cotyledons. He also dealt with the general anatomy of fruits and seeds; he was the first to discover pitted vasa in secondary wood, and the first also to see crystals in plant tissues in the rhizome of the Iris.

The Franklands, in their Life of Pasteur, state that Leeuwenhoek in 1675 discovered yeast-cells in the deposit under a fermenting liquid. He wrote on the forms of crystals, of sugar in particular, on the microscopic appearance of the edge of razors, and he left directions as to how to hone them for shaving.

One feature in Leeuwenhoek's work which does not seem to be as widely known as it ought to be is his battle against the all-prevailing superstitions of his time regarding "spontaneous generation" or Abiogenesis.

It is difficult for us at the present day to realize the depth of darkness and confusion amid which men's minds worked in the seventeenth century with regard to the subject of generation. The Cochineal insect was supposed to be "the fruit of some tree," the putrefaction of the carcase was supposed to engender maggots, the putrefaction of meat to give rise to flies, insects found within galls were supposed to have arisen from the tissues of the tree, even the otherwise enlightened Redi thought this, the flea was believed to be bred of "corruption" in dust or sand, etc., marine molluscs were imagined to be produced from marine mud, and eels were supposed not only by the vulgar but also by learned men to be produced from dew or in a manner wholly inexplicable.

Leeuwenhoek was in no doubt whatever as to these things being prodigious absurdities bred of a priori zoology and Aristotle. "For my part," he says, "I hold it equally impossible for a small shell-fish to be produced without generation as for a whale to have its origin in the mud." He had risen in a manner that a great many people at the present day have not risen, to a grasp of the principle that size has nothing at all to do with the essence of phenomena; that the antecedent of a living thing is a living thing and not non-living matter or dead matter however putrid it may be.

He demonstrated to his daughter and to his engraver the embryo of Unio, the fresh-water mussel. They "watched it for three whole hours." He was particularly delighted with his discovery of the embryo eels in the body of the female, for he had been challenged specifically to show how eels were bred.

The superstitions about spontaneous generation died very hard. Well on into the eighteenth century we have Father Needham (1748), introducing sterilization, and later Spallanzani (1776) bringing fresh proofs against Abiogenesis. Cagnard dela Tour (1837), Schwann (1839) and Helmholtz worked successfully at the same problems. But the coffin of this dead heresy was rapidly approaching completion; Leeuwenhoek had found the wood, the others had put it together, while it was reserved for Pasteur and Tyndall in our own day to hammer in the last two nails with triumphant blows.

In the case of "weevils" of granaries, he showed that they were not produced by wheat, but were grubs from insects' eggs. He suggested that the granaries should from time to time be fumigated with sulphur to destroy the insects before they laid their eggs. This is quite in the modern spirit, he wished to use some sort of chemical to destroy life. In other words, he is not the mere histologist magnifying "the difficulty, looking it full in the face and passing on," but he is a physiologist as well trying to solve it.

He knew that the atmospheric air was necessary to maintain life, even the life of parasites on trees. He writes in 1700 of a free circulation of air as being necessary for insects which he had imprisoned in a glass tube for observation. In fact, he understood ventilation as few in his day did, except Stephen Hales, and had people understood it earlier there would have been less of gaol-fever and typhus fever everywhere, and poisoning of people to death on board ship. Leeuwenhoek was also something of a physiologist for he speculated on the use of the lacteals, of the "slime of the guts," and made an experiment to determine the amount of aqueous vapor daily excreted from the skin. He weighed a glass vessel dry, and, inserting his hand into it and closing up the orifice by a handkerchief packed round the wrist, he found moisture condensed on the inside of it. On weighing the vessel at once before the moisture evaporated, he obtained a distinct increase of weight, and by knowing the area of skin that had produced this, he arrived at the amount of sweat excreted per unit of surface-area. He then estimated the total area of the skin and found that the whole body per 24 hours must eliminate about 28 fluid ounces, which is not very far from the truth.

Leeuwenhoek was evidently possessed of sound commonsense. He could not bring himself to believe that the blood underwent any kind of a "fermentation" analogous to that of beer or wine as was at the time almost universally taught in the medical schools. He said very sensibly that he had never been able to see any bubbles of gas in the blood-vessels such as ought to have been there had the blood been the seat of fermentation as he understood it. However necessary this fermentation of blood was to the theoretical physiology of his day,

that Leeuwenhoek could find no evidence of it was a serious objection seeing that he was one of the very few who was in the habit of looking at the living blood. Thus, although not a physician, and not even a professional man of science, he was considerably ahead of the physiology of his day in respect of some of its most important principles.

Leeuwenhoek is not forgotten in his own country, for the Royal Society of Amsterdam gives a medal once in ten years in recognition of the most important work done on microscopic organisms during the previous decade. In 1915 this medal was awarded to Major-General Sir David Bruce, Director of the Royal Society's commission for the investigation of the sleeping sickness.

As a lover of pure science, as a most industrious worker with the microscope, and as a prescient physiologist Leeuwenhoek is interesting to all those who study living nature, but by histologists and bacteriologists in particular his name will ever be regarded with that reverence which a bright example in science must always inspire. The man who first saw and described a micro-organism cannot fail to be interesting to us today seeing of what importance the microscope and its dependent science of bacteriology have become in modern life with their relations to applied medicine, pathology, surgery, public health, the arts, and commerce. The seed of this gigantic tree with its roots in all lands and its flowers culled under every clime was sown by the hand of a solitary worker without reward in a quiet little town on one of the canals of the great plain of Holland.

STONE AGE MAN'S CURE FOR HEADACHE

By Professor ROY L. MOODIE
UNIVERSITY OF ILLINOIS

HEADACHES are bothersome things. People have been annoyed by them for a long time, seven thousand years at least, and probably longer. We are all quite willing, when afflicted with a headache, to agree with the people of the old stone age that a headache is a demon and we would be willing to do most anything to get rid of it. The pain whether due to a blow on the head, indigestion, nervousness or other cause certainly reminds one of a demon and it is readily understood how ancient man should have conceived the idea of releasing this demon which was bothering him. He devised a remedy which certainly was an effective cure for headache whether the pain was due to eye-strain, brain tumor, skull fracture or nervousness, although it must be admitted that his cure was worse than the pain.

Primitive man devised his curative measures as a phase of his religious beliefs, hence the cure adopted for headaches was a religious rite. The operation was performed by a shaman or medicine man in some remote fastness of his region and here the patient remained until completely recovered. This treatment consisted in opening the skull in a variety of ways to relieve the pain, or, as the stone age men thought, to let out the demon. Men in the stone age phase of their culture whether in Peru, Mexico, France, Kabylia or the South Sea Islands practiced this method of relief and it is said to be still employed in the highlands of Peru and Bolivia and in northern Africa.

This ancient surgical art, which forms the very beginnings of prehistoric surgery, seems to have been developed first in the region just north of Paris near the Seine and Oise rivers some seven or eight thousand years ago. In the dolmens, or burial mounds, scientists have found the ancient skulls of people who had suffered headache and who had had their skulls trepanned or opened to release the headache demon. No special class of individuals seems to have been favored since the operation was performed on man, woman and child, apparently without respect to either age or sex. Its frequency is attested by the great number of skulls exhibiting the surgical openings. In one burial mound in France yielding the bones of 120 individuals more than 40 showed the effects of trepannation.

SKULL OF ANCIENT INDIAN WHO SUFFERED HEADACHE. THE RESULT OF THE "CURE" BY THE WITCH-DOCTOR IS SEEN IN THE FOREHEAD, WHERE THE BONE HAD BEEN SCRAPED AWAY BY A SHARP FLAKE OF STONE. SUCH SKULLS ARE COMMONLY FOUND IN PERU.

It isn't very pleasant to picture the torture undergone by the ancient sufferer at the hands of the priest who either cut, scraped or bored the bone of the skull away with a sharp piece of stone. Some relief from pain may have been had by the application of a quid of coca, a plant yielding anesthetic substances which grows in Peru. But the worst thing about headaches is that they recur, so the ancient people not deterred by one failure submitted themselves to the operation again and again. A few ancient skulls reveal five cruel openings, which had all healed. The patient had survived them all.

The equipment of the primitive surgeon was meager. His knowledge of cleanliness was not keen. If he possessed a rough flint knife, a scraper, a few leaves of the coca plant and a piece of coarse cloth to bind the wound he was content. A mossy bank out in the woods served him for an operating table.

CAN THE ALASKA SALMON FISHERIES BE SAVED?

By BARTON WARREN EVERMANN
DIRECTOR, MUSEUM CALIFORNIA ACADEMY OF SCIENCES

THERE are, in Alaskan waters, five species of salmon, all belonging to the genus *Oncorhynchus*. They are (1) the King, Chinook, Quinnat, or Spring Salmon (*Oncorhynchus tschawytscha*); (2) the Sockeye, Red, or Blueback Salmon (*O. nerka*); (3) the Coho, Silver, or White Salmon (*O. kisutch*); (4) the Humpback, or Pink Salmon (*O. gorbuscha*); and (5) the Dog, or Keta Salmon (*O. keta*). The Steelhead Trout (*Salmo gairdneri*) also occurs in Alaska where it is usually classed as a salmon by the commercial fishermen.

Each of these six species is more or less abundant in Alaska and all are the objects of important commercial fisheries.

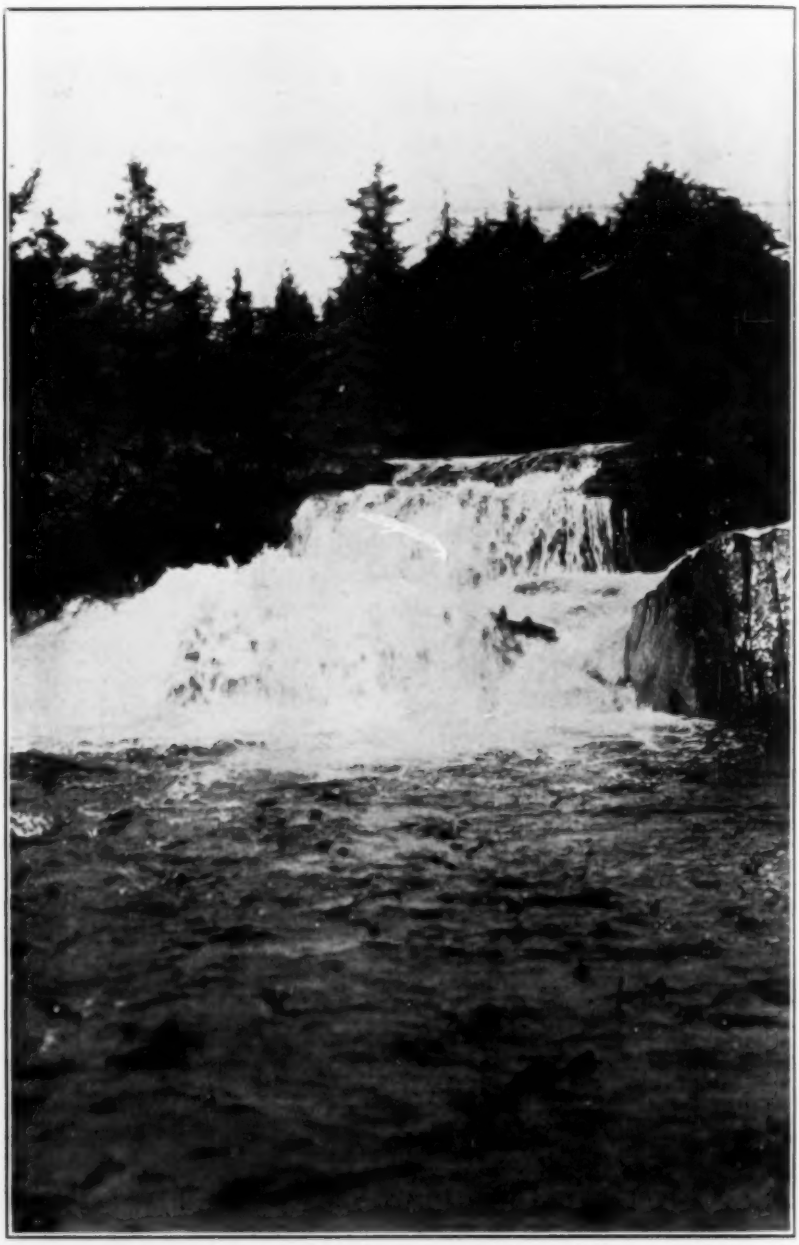
Habits of the Pacific Salmon

The habits of all the different species of Pacific salmon are essentially the same. They are all anadromous; that is, they live most of their life in the sea and enter fresh water only to deposit their eggs. After having spawned once they all die, both males and females alike; none lives to return to salt water. The eggs are deposited in the gravel or on other suitable bottom, usually in the fall of the year, and usually well toward the headwaters of freshwater streams. They hatch in the winter and following spring, but not until some time after the fish that produced them have died. There is a period of a few weeks each year during which each particular salmon family is represented only by a number of eggs. Both parents are dead and none of the children has yet been born; there are only eggs to tide the family over. It is, therefore, evident that no Pacific Coast salmon ever saw either of its parents or any of its children.

After the eggs have hatched the young migrate to the sea, some going down as fry while others remain in freshwater at least one year. In the sea they live and grow rapidly and, when mature, return to freshwater where they spawn and die, thus completing the life-cycle.

The Sockeye or Red Salmon

In the present paper, consideration is given to the Sockeye Salmon only. In order that the recommendations that will be presented may have a proper background of fact, and that they may the more readily be understood and appreciated, it will be well to give in somewhat greater detail some of the more important facts in the life-history of



CASCADES IN LITNIK STREAM, AFOGNAK ISLAND, SHOWING SALMON STRUGGLING IN THE FALLS AND IN THE POOL BELOW. NEAR HERE THE BUREAU OF FISHERIES HAS OPERATED A RED SALMON HATCHERY FOR MANY YEARS WITH UNCERTAIN RESULTS

this very important fish, particularly those wherein it differs from the other species.

The Sockeye enters Only Those Streams that have Lakes in their headwaters

Unlike the other species of salmon, the Sockeye enters only those streams which have one or more lakes somewhere in their course. It ascends such streams to the lakes and spawns either in the shallow water near the shores of the lakes themselves or, more commonly, in the small streams that flow into the lakes. So far as is known, there is no exception to this rule.

Upon reaching the spawning beds, the sockeyes deposit their eggs in the gravel bed of the stream or lake where, more or less deeply covered by the gravel, they remain for several weeks before they hatch. Then the young go down to sea, as fry the first spring after hatching, or they may remain in the lake until the second spring and then go down as yearlings or fingerlings.

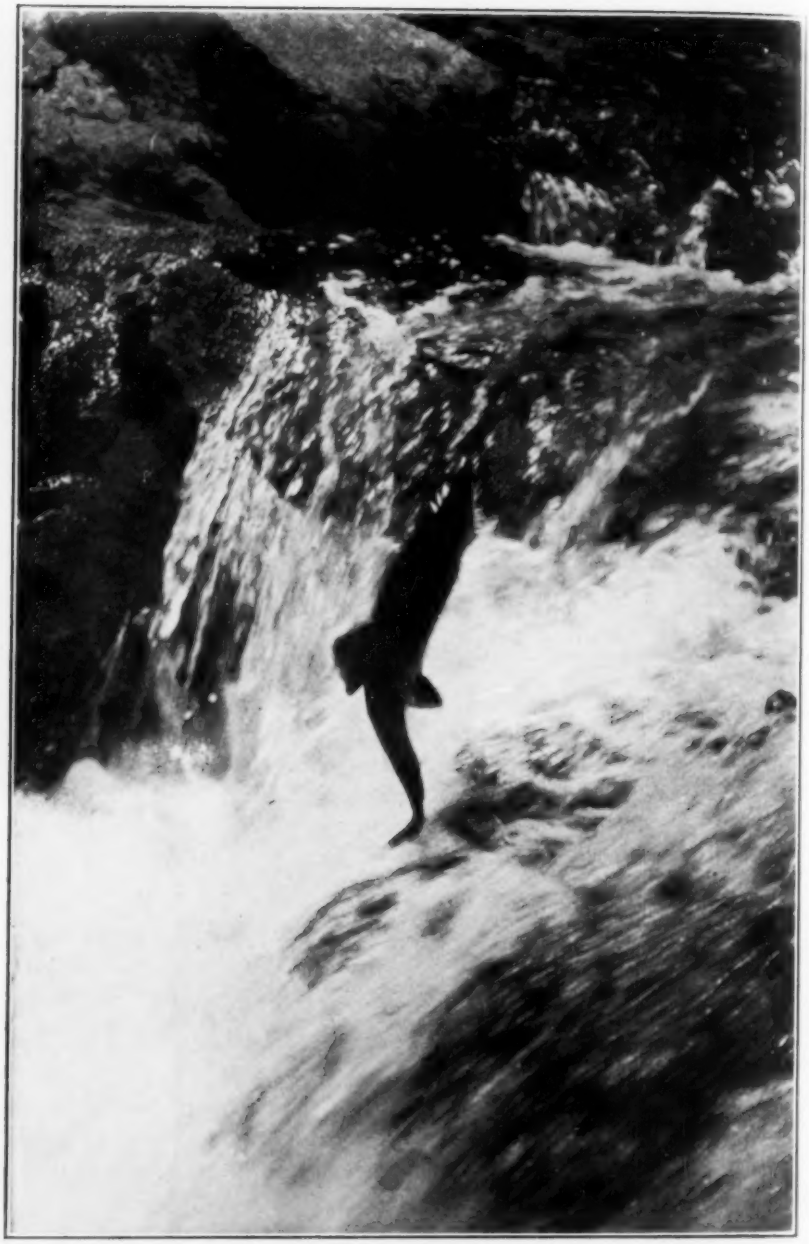
The run of adult sockeyes into freshwater varies considerably in different streams. It may occur at any time from spring until fall. The actual spawning takes place in the late summer or fall. The eggs hatch in the fall, winter, or early spring following.

Age at which the Young Sockeyes Migrate to Salt Water

Chamberlain showed (1903-1905) that Sockeye Salmon fry hatched at the Fortmann hatchery linger a short time on the nursery grounds where liberated, then move down into the lake where they remain a year before going on down to salt water. Gilbert's observations have confirmed those made by Chamberlain, especially as applied to the smaller streams. In the larger rivers, many go down to sea the first spring after hatching, while others remain in the lake until the second spring.

Parent Stream Theory

It has long been maintained by salmon fishermen and others that salmon, when mature, usually, if not invariably, return to the particular stream in which they were hatched. It was generally believed that a great majority of the fish hatched in any particular stream would return to that identical stream when mature and ready to spawn, but that a good many would, or might, go to other streams. Some thought that the salmon return to their own stream because they possess a marvelous geographic or homing instinct, while others maintained that the salmon, after going down to sea as fry or fingerlings, do not wander far from the mouth of the stream in which they were hatched, and that. when they reach maturity they seek freshwater; and the fresh-



A REALLY REMARKABLE PHOTOGRAPH OF A HUMPBACK SALMON ASCENDING THE FALLS IN LITNIK STREAM, AFOGNAK ISLAND

water most easily found is that nearest at hand, which is the water of the stream in which they were hatched; they therefore ascend that particular stream.

While this theory, known as the "Parent Stream Theory," has long been held by many, it was not until recently that its truth was demonstrated. As early as 1906, Chamberlain announced, as one of the conclusions reached from his study of the Sockeye at Naha Stream, Yes Bay and elsewhere in Alaska, that "at least the greater part of the supply of any stream must be derived from the fry produced in that stream."[1]

As one of the results of a long series of investigations and observations requiring infinite patience, as well as the greatest care and skill, Dr. Gilbert has demonstrated the validity of the theory not only in its essential features but even in its minutest details. Gilbert says:[2] "The validity of this important theory has been conclusively demonstrated in the case of the larger rivers of the Province. * * * Examination of the scales has removed any possibility of doubt that the progeny of the Fraser River fish return to the Fraser at their maturity, and that this is true also of the fish of each of the large river-basins. It has now been shown * * * that this same principle holds in the case of all the rivers and creeks, however small these may be, and however near together they may enter the sea."

Gilbert's study of the scales has shown that the salmon of each particular stream possess scale characters in common which enable them to be distinguished from the salmon of any other stream, however near the streams may be to each other. He calls attention to the fact that it is only during their life in freshwater that the salmon are subject to obviously diverse conditions. He further says:[3] "It frequently happens that two lakes belonging to different river systems are separated by a few miles only across a low divide. Their physical conditions, it can not be doubted, in so far as these depend on climate, are practically identical. Yet the sockeyes they produce grow each after its own kind while still in freshwater, and exhibit characteristics of growth and habit which distinguish them from their near neighbors across the divide, and ally them closely with all the other like-colonies of their river-basins, however distant these may be."

Life-History of Sockeye Salmon as Recorded in Their Scales

The study of the life-history of a salmon, as recorded in its scales, presents one of the most fascinating of stories. There is none more marvelous in all animate nature. Suppose you should take a trip to

[1]Some Observations on Salmon and Trout of Alaska, Bureau Fisheries Doc. 627, 1906 (1907), p. 25.

[2]Contributions to the Life-history of the Sockeye Salmon (Paper No. 3), B. C. Fisheries Department, 1915, p. 527.

[3]Loc. cit., p. 3.

HUMPBACK SALMON ASCENDING UYAK FALLS, AFOGNAK ISLAND

British Columbia and Alaska this summer, and while there you should visit some of the great salmon canneries. You have heard somewhere that the age of a salmon can be told from its scales. But you are skeptical. To try the thing out, you cut off a piece of skin with a few scales on it from the side of each of three or four salmon at three or four different canneries. You ask the cannery superintendents from what streams those particular fish came. They tell you and you make a note of it. Numbering the samples for identification, you send them to some expert, Dr. Gilbert, let us say, and ask him to examine them and tell you how old each of the fish was and where it came from. When you get Dr. Gilbert's report you will be surprised. He will tell you not only how old each fish was, but he will tell also the particular stream from which it came, if, perchance, it came from a stream whose salmon he has studied. And he will tell you how long it remained in fresh water before it migrated to the ocean—whether it went down to sea while yet in the fry stage the first spring after hatching, or remained in freshwater a year longer and then went down to sea as a yearling or fingerling. If it remained in the lake one summer and one winter, he will tell you whether the summer was a favorable one as to food supply and other conditions which enabled it to grow rapidly. He will tell you how long it lived in the sea—how many summers and how many winters, and this, of course, added to the time it spent in freshwater before going to sea, will give its age. And, finally, he will tell you what stream this particular salmon was bound for when it was caught. He will be able to learn all these facts regarding each of the specimens you submit to him from an examination of the scales.

Age at which Sockeye Salmon mature

It has been shown by Gilbert that the Alaska and British Columbia sockeyes, as a rule, mature and return to their home streams to deposit their eggs when four years old. A considerable proportion, however, in certain streams, do not return until five years old, and still smaller numbers return at three, six, or even seven years of age. This varies with different streams. For example, it was found that the great majority of the Fraser River sockeyes mature at four years of age, but that, in average years, 10 to 15 per cent. of the run consists of five-year fish. As a result of his study of the sockeyes of Naha Stream, Chamberlain concluded that they are chiefly four-year fish. The Nushagak Bay salmon are either four-year or five-year fish, chiefly the latter.

Attention should here be called to the fact that the salmon return to the stream in which they were liberated when young and in which they were reared through the fry stage or longer. Under natural conditions this, of course, will be the stream in which were laid the eggs

KARLUK HATCHERY, SHOWING RETAINING CORRALS

which produced them. But when eggs taken in one stream, as, for example, the Yes Bay Stream, are hatched and liberated as fry or fingerlings in another, as the Columbia, for example, they will return not to Yes Bay but to the Columbia.

As the adult salmon return to the stream in which they as young were liberated and reared, regardless of whether the eggs from which they were hatched came from fish of that stream or not, it is evident that there is no inherited "homing instinct." They return to the stream in which they, as fry, fed, and not to the stream in which their parents fed when fry, unless it be the same stream. The name "Parent Stream Theory" is not well chosen. "Home Stream Theory" is suggested as a better name.

A TYPICAL WELL-EQUIPPED SALMON CANNERY ON NUSHAGAK BAY. A CANNERY OF THIS TYPE MEANS AN INVESTMENT OF MANY THOUSAND DOLLARS

In view of this important fact, a hatchery which liberates all its output in the stream on which it is located will have no effect upon the run of salmon in any other stream. As Gilbert has said: "In order to maintain the supply of salmon in a given district, it will not be adequate to install a hatchery on any convenient stream into which the entire output of the hatchery will be turned. On the contrary, each stream must be given separate consideration, and must receive its own quota of fry to grow within its boundaries. The original source of the eggs is seemingly a matter of no importance. The destination of the adult salmon is determined by the locality in which the young were reared."[1]

Importance of the Alaska Sockeye Salmon Fisheries

The importance of the Alaska Sockeye Salmon fishery has been very great, as every one knows. As Professor Cobb has said: "Alaska is the most favored salmon-fishing region in the world." The first cannery was erected in 1878 and the first pack was made that year. Other canneries were established from time to time until the number operated in 1918 was 135. The annual pack has varied from 8,159 cases in 1878 to 2,201,643 cases in 1914, when the zenith seems to have been reached. Since 1914 the pack has suffered a great decrease, the pack in 1919 being only 1,204,343 cases. The total pack from 1878 to 1919 reached the enormous total of about forty million cases, as shown by years in tables which have been compiled by Professor Cobb.[1]

Artificial Propagation of the Sockeye Salmon in Alaska

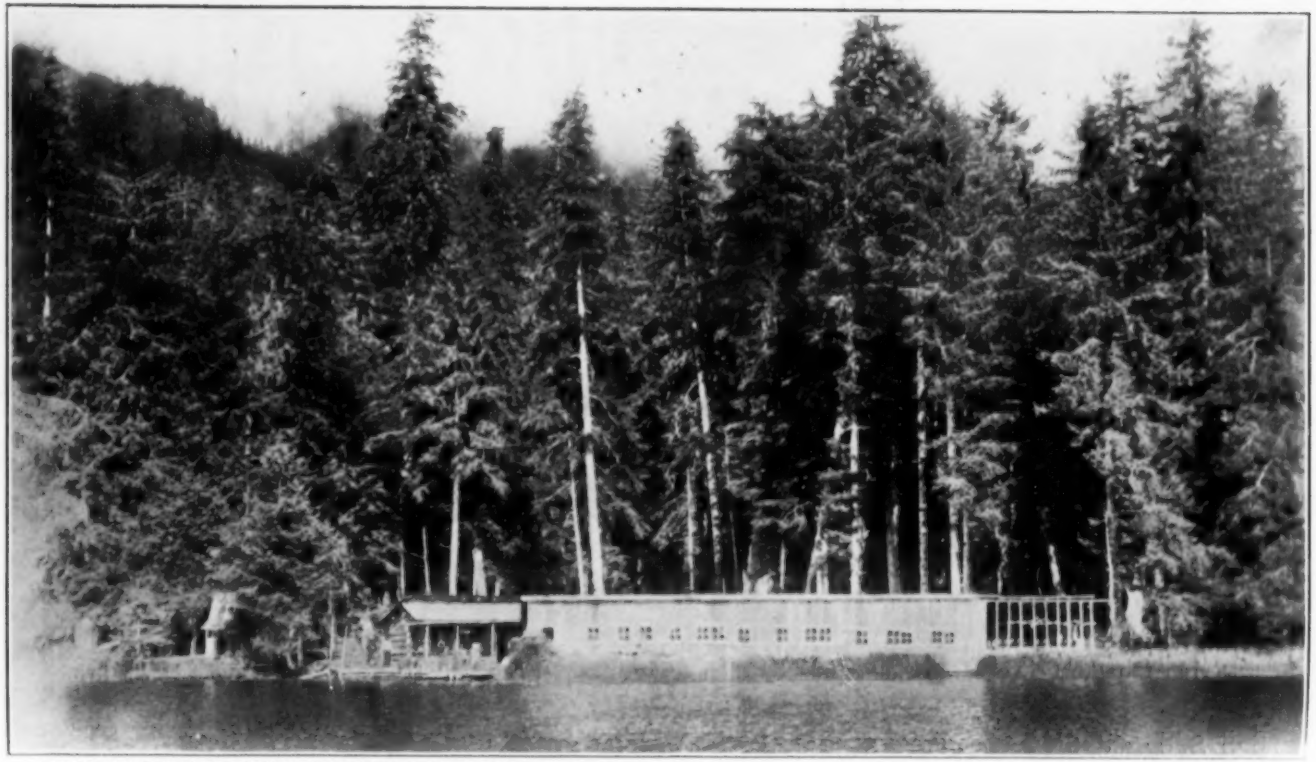
The artificial hatching of salmon in Alaska was begun about thirty years ago. Private hatcheries have been operated at several different places, as at Etolin Island, Karluk, Hetta, Quadra, Freshwater Bay, Kell Bay and Naha. Federal hatcheries are maintained at Yes Bay and at Afognak.

The Callbreath Hatchery

One of the most interesting of these hatcheries was that maintained by Captain Callbreath on Jadjeska Stream, Etolin Island. Captain Callbreath had full faith in the parent stream theory. He believed that, through artificial propagation, he could build up a large annual run of salmon in this small stream. He began in 1893 and continued the experiment until 1905. His method was to catch all the sockeyes ascending this stream, take their eggs and, after hatching, release the fry in the stream.

The experiment was continued for a period of thirteen years. Captain Callbreath, always hopeful, abandoned it only when total

[1]Gilbert, 1915, No. 3, p. s 327.
[1]Report U. S. Fish Commission for 1916, pp. 156-181.



ETOLIN ISLAND RED SALMON HATCHERY WHICH CAPTAIN CALLBREATH OPERATED FOR MANY YEARS IN AN UNSUCCESSFUL ATTEMPT TO ESTABLISH A LARGE RUN OF SALMON IN A STREAM IN WHICH BUT FEW SALMON NATURALLY RAN

blindness and the infirmities of age made it impossible for him to continue longer. He liberated a total of more than 52,000,000 fry. He at first believed the results would show in four years. But there was no increase in the fourth year. Then he expected them in the fifth year, but was again disappointed. Always optimistic, he never lost faith as to the final result. He extended the time to six years, then to seven, then to eight, nine, ten and even longer. The increased run for which he had labored and of which he had dreamed for so many years, never came, and Captain Callbreath died still believing the big runs would begin "next year." Not only did the run in Jadjeska Stream fail to show any increase, it actually diminished. A more pathetic story is not known in all the history of the Alaska salmon fisheries.

The Fortmann Hatchery

The Fortmann hatchery on Naha Stream near Loring, Southeast Alaska, was established by the Alaska Packers Association in 1901, and has been operated each year since. It is the best equipped and largest salmon hatchery in the world, its capacity being over 110,000,000 eggs.

Since the establishment of this hatchery no fishing has been permitted off Naha Stream. The entire run has been permitted to enter the stream and all the fish have been utilized by the hatchery. The fry have been liberated in the lakes of the Naha system. It is assumed that all the eggs that could be obtained were taken; yet, instead of there being an increase in the run, as was expected, there has been a marked decrease. There has been a decided progressive decrease in the number of eggs taken in each of the last three five-year periods, the figures being:

1905-1909	245,674,000
1911-1915	197,580,000
1916-1920	133,980,000

These figures show that the number of eggs taken in the last 5-year period (1916-1920) were less than 56 per cent. of the number taken in the first full 5-year period (1905-1909). Or estimating the egg take of the present year (1920) at the average for the preceding four years, we then have four 5-year periods to compare, and find that the take in the last period is 64 per cent. of that in the first, 51 per cent. of the second, and 70 per cent. of that of the third.

The number of red salmon eggs taken by the various hatcheries totals nearly three billions. If the fish that supplied this enormous number of eggs had been permitted to spawn naturally, the number of eggs they would have produced would doubtless have been much greater, for, in artificial spawning, there is always considerable loss. It is a fair question to ask what would have been the result if these fish had been permitted to spawn naturally?

EGLIN ISLAND RED SALMON HATCHERY WHICH CAPTAIN CALLBREATH OPERATED FOR MANY YEARS IN AN UNSUCCESSFUL ATTEMPT TO ESTABLISH A LARGE RUN OF SALMON IN A STREAM IN WHICH BUT FEW SALMON NATURALLY RAN

Statistical tables and other available data, when examined in their relation to each other, ought to throw some light upon the question of the actual value of artificial propagation of the Sockeye Salmon and its relative value as compared with natural reproduction. Unfortunately the records have not been kept with the fullness and accuracy essential to unquestioned conclusions on all phases of the problem, but it would seem that the experiment has been conducted sufficiently long and in a sufficient number of places to warrant the raising of the question as to whether artificial propagation of salmon in Alaska, as conducted, has been more effective in conserving the salmon fisheries than natural reproduction would have been. In answer to the question as to the effect of the Yes Bay hatchery upon the salmon fisheries of Alaska, the U. S. Commissioner of Fisheries replied: "It is difficult to furnish direct evidence of the effects of the [Yes Bay] hatchery operations other than what may be indicated by the statistical figures." Essentially the same reply was given regarding the Afognak hatchery.

In reply to questions as to what increase, if any, has been noted in the catch of salmon in the region with which the Fortmann hatchery is concerned, the Alaska Packers Association replied that no data are available. "We have no data * * * therefore can form no judgment what effect the output of the Fortmann hatchery and the near-by United States Government Yes Bay hatchery may have had upon the supply of salmon of that district."

As to the Karluk hatchery, it is quite certain that none of the millions of fry liberated at that station ever survived to maturity. Although the Karluk run continued large for many years, it was doubtless able to do so because of a considerable annual escape of fish to the spawning beds in and about Karluk Lake where they spawned naturally.

Sockeye Salmon Streams on which Hatcheries have not been operated

There are many Red Salmon streams in Alaska which have not had hatcheries located on them and which, therefore, have not been influenced one way or the other by artificial propagation. Among important streams of this kind that may be mentioned are the Alitak, the Chignik, and the *Nushagak* region. Each of these has supported a great fishery for many years. Although enormous catches were made year after year and although it was felt that the supply must fail sooner or later, there was little decrease for many years. The enormous run in each of these streams depended entirely upon natural reproduction; there was no hatchery on any of them. I have not at hand the figures, but it seems that these streams were able to hold up under natural spawning longer than those streams where artificial propagation was resorted to. It is not claimed that a direct comparison

of the value of natural reproduction as compared with artificial propagation as exhibited by these streams is by any means conclusive, but it is believed such comparison as can be made fails to demonstrate any marked advantage of artificial salmon culture, *as conducted*, over natural methods.

I do not claim for a moment that the artificial propagation of salmon in Alaska must necessarily be less effective than natural reproduction, but I do say that its superiority has not been demonstrated. Indeed, the available evidence would seem to justify the statement that, up to the present, the salmon hatcheries in Alaska have done more harm than good. It would surely be unwise to establish additional hatcheries in Alaska until investigation has shown wherein lies the defect in present methods. The whole question of method in hatchery work and fry planting needs the most careful scientific study by the very best men that can be induced to undertake the solution of this vital problem.

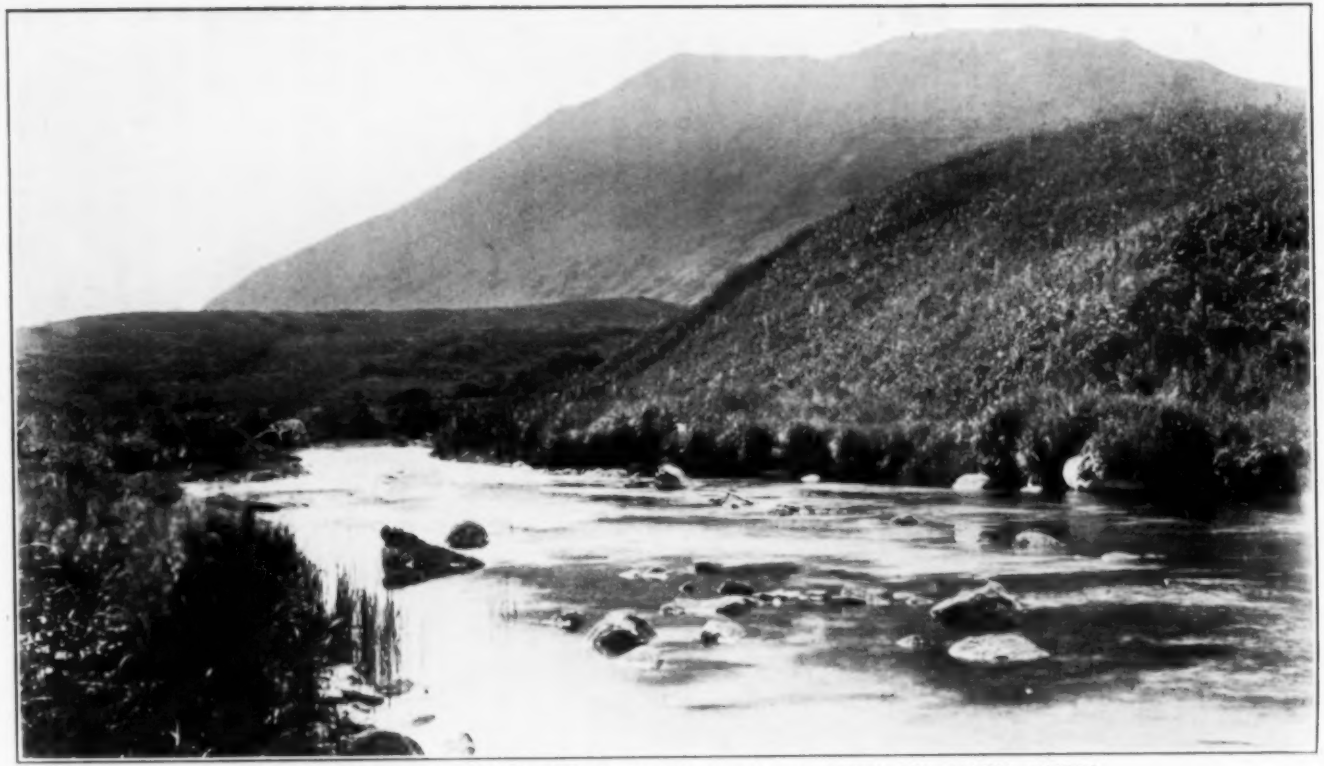
How can the Alaska Salmon Fisheries be saved?

That the Alaska salmon fisheries, as well as those of Puget Sound and Fraser River, are doomed unless something is done and done soon can not be doubted. They have already in some streams reached a condition of depletion so complete that commercial rehabilitation will be possible only with the complete stopping of all fishing for a long period of years. But that this great industry can be saved I have not the slightest doubt.

The knowledge we now have of the life-history of the Sockeye salmon, acquired chiefly through the painstaking investigations and study made by Dr. Gilbert, is adequate for the formulation of a definite plan of investigation which will supply the data which will point the way to a method of conducting the salmon fishery so that we shall not only be able to rehabilitate it but to continue it indefinitely as a going industry with a maximum annual output. Indeed, we can even now say with confidence what the essential features of the plan must be. They are five in number:

1. *Determine the ratio between the number of salmon spawning naturally and the run four (or five) years later.* In other words, determine the number of adult fish which may be expected to return at the end of the four—or five-year—cycle as the result of the natural spawning of a certain number of fish.

   There has been much discussion as to the number of salmon eggs required to produce one adult salmon; in other words, for every salmon that spawns naturally on the spawning beds, how many adult salmon may be expected to return four or five years later?



HEAD OF NORTH OLGA STREAM, ALITAK, KODIAK ISLAND; A TYPICAL RED SALMON STREAM

In 1908, the Bureau of Fisheries began a series of observations on a certain stream in Alaska. The observations were known as *The Wood River Investigations*. The present writer had for some years been disposed to believe that, if the run of salmon in a suitable stream could be studied in the proper way for the requisite number of years, the problem could be solved. Wood River in the *Nushagak* region was selected as the stream on which to make the study, and the investigation was begun in the summer of 1908.

A rack was placed across Wood River at the outlet of *Lake Aleknagik* in such a manner as to prevent any salmon from entering the lake except as they were permitted to pass through gates so constructed that they could be opened or closed as desired. A suitable platform or observation station was constructed above the gates from which the observer was able to open or close the gates at will and count the salmon as they passed through. It was found that the fish passed through freely and in an orderly manner, so that an intelligent, conscientious attendant, after a little experience, had no difficulty in keeping an accurate tally. The method was very simple. An actual count was made of the fish passing through in one minute in each fifteen minutes. The number counted in the one minute was assumed to be the average for 15 minutes.

This investigation was carried on every year from 1908 to 1919, both inclusive, except in 1914, when the Secretary of Commerce in his wisdom refused to continue it. He had little or no sympathy with scientific investigation or appreciation of its value. The

A TYPICAL SALMON TRAP NEAR GRAVEYARD POINT, KIRCHAK BAY, SHOWING THE NORTHERN AND INSHORE POT, AS SEEN FROM THE BEACH AT LOW TIDE, THE TUNNEL BEING DOWN

NATIVE METHOD OF USING GILL NETS BETWEEN TIDES ON THE BEACH IN NUSHAGAK BAY

continuity of the investigation was thus broken and the whole experiment rendered of little value.

If an investigation of this kind is carried on for a sufficient number of years on a stream which does not present any complicating conditions which would render the results inconclusive, and, if, during the same years, an accurate record is kept of the fish taken by the canneries from the run of that stream, there would be acquired all the data necessary to answer the question as to how many fish must be permitted to spawn each year in order to provide a certain catch year after year.

When Wood River was selected for the purpose of this experiment, certain difficulties were recognized as existing. Wood River is only one of four streams that enter Nushagak Bay. The other three are the Nushagak, the Egushik, and the Snake. The mouths of these four streams are not far from each other. Each of them receives a part of the Nushagak Bay run. What proportion of the entire run goes to Wood River, and what to each of the other streams no one knows, but it is certain that the Wood River contingent is vastly greater than that of the three other streams combined; indeed, it was thought the run in the Nushagak, Egushik and Snake was relatively so small as to be negliable for the purpose in view. It was believed that a census of the escape in Wood River taken for a series of eight or ten years, compared with the commercial catch for the same series of years, would enable us to arrive at an approximate ratio of escape to catch that would be fairly reliable; in other words, we would know how many fish must spawn in Wood River each year to permit a certain catch four and five years later.

The safety of this assumption may well be questioned. But we know more about the Red Salmon now than was known then. We now know that it is quite practicable to determine the proportional run in each of the four rivers without racking any but Wood River. Dr. Gilbert has shown that salmon return to the particular stream in which they were hatched and that the salmon of any stream are readily distinguished from those of any other stream however close together may be the mouths of the two streams. This being so, it is only necessary to determine what are the distinguishing characters of the salmon of each of the four streams—the Wood, the *Nushagak*, the Egushik and the Snake, then an examination of a considerable number of salmon at each Nushagak Bay cannery for a series of years would enable one to arrive at the percentage of fish of each river in the entire run. The actual escape of Wood River fish is learned by the census, and the percentages obtained at the canneries would enable one to arrive at the escape to each of the other streams.

THE RACK AT LAKE ALEKNAGIK (WOOD RIVER) JULY, 1912

In view of these facts, it seems that Wood River offers an entirely practicable field in which to solve this vitally important problem.

It is realized that the ratio determined for Wood River may not be the proper ratio for any other salmon stream. It will in all probability be necessary to determine the ratio for each particular stream. A census should be made on every red salmon

THE RACK AT LAKE ALEKMAGIK (WOOD RIVER) JULY, 1912

stream possible. There are doubtless some streams on which this investigation can not easily be made, but the great majority of Alaska red salmon streams present no insuperable difficulties, and probably the ratio for all can be determined if the rack is put further up the stream. In any event, the data which can be gotten on the other streams will suggest what to do with those few where it is impracticable to determine the ratio.

COUNTING RED SALMON AS THEY PASS THROUGH THE RACK AT WOOD RIVER, JULY, 1912

2. *Study of the spawning beds.*—The spawning beds in every red salmon stream in Alaska should be carefully studied for the purpose of determining just what conditions are favorable and what unfavorable; what percentage of the eggs are fertilized; what percentage hatches; what percentage of the fry escape all enemies and go down to sea; what the natural enemies are and how destructive each is; whether there is over-crowding of spawning fish, and the result; depth to which the eggs are buried and what the best depth is; food supply for the fry, qualitative and quantitative; and any and all other questions which throw any light on the essential habits of the adults and young when in fresh water. We know practically nothing about the efficiency of natural reproduction of the Sockeye. Fish culturists declare unhesitatingly that a large percentage of the eggs fail of fertilization; that a large percentage of the eggs that do succeed in getting fertilized are lost by being washed away, by being crushed, by being eaten by fishes or other enemies, or by disease of one kind or another; and that of those that do hatch only a small percentage

escape the various enemies that beset them in their fry or fingerling stage, and grow to maturity. All of this in a sense seems reasonable and may be true, but nobody knows; and certainly no convincing proof has been presented. Indeed, the question has never been seriously investigated. The asseverations of the fish-culturists are merely their opinions, supported by scarcely any known facts.

But we should know. We should determine through repeated observation and investigation, what the normal or average loss is from failure in fertilization, from mechanical injuries, from fish and other enemies, from disease, and from all other causes. Else we shall never be able to compare natural with artificial propagation.

3. *Improvement of spawning beds.*—Much can doubtless be done toward improving the spawning beds by removing obstructions, by bettering the character of the gravel, and by increasing the area. It is believed that improvements can be made in many places which will permit the spawning of a much greater number of salmon than now find room or suitable bottom. To know the capacity of any stream it is necessary to know the extent of bottom suitable for spawning beds, whether the beds are utilized to their full capacity, and whether the beds can be improved by enlargement or otherwise. It may well be that, in some streams, the spawning fish are too crowded on parts of the bed while other parts are only partially utilized. It may well be that, by a little work the character of some beds can be improved and that, through removal of obstructions the area of suitable bottom can be greatly increased. It is directly in the interest of the salmon fisheries that every Sockeye salmon stream in Alaska be physically improved to its maximum area of suitable spawning bottom and that, within a margin of safety, spawning fish be permitted to reach those beds to their full capacity.

4. *The life of the young while yet in fresh water.*—As already stated elsewhere in this paper Dr. Gilbert and Mr. Chamberlain discovered that some of the young migrate to salt water in their first spring, others in their second, the proportion varying with different streams and different seasons. While in freshwater the fry may encounter conditions more or less adverse. The food supply may be inadequate, and living enemies such as salmon trout, bullheads, terns, and other birds, and other kinds of fishes, may make serious inroads on their numbers. All these factors should be carefully studied and the conditions improved wherever possible.

5. *Statistics of the run of salmon.*—During the years that the studies outlined above are being made on the streams, statistics of the run of salmon of each stream should be made. The primary object of these studies would be to determine the total number of fish of each particular stream for each of a series of years. If the investigation is properly made, the total run of each stream would be the number of fish headed for that stream taken by the fishermen, plus the escapement as determined by the census at the rack, plus a negligible few that escaped observation. These data, for a series of years would, it is believed, supply a ratio between the number of fish in the escape and the total run which will show the number of fish that must be permitted to escape to the spawning beds to maintain the fishery at a maximum annual pack.

SUMMARY

The essential features of the plan here proposed for the rehabilitation and conservation of the Alaska salmon fisheries may then be summarized as follows:

1. Rack every stream that can be racked and make an accurate count of the salmon ascending it. The census or count must be made often enough to cover at least two returns of each annual run of the first two or three years.

2. In each of the years in which a census of the spawning fish is made, secure statistics showing the number of fish taken by the fisheries from the run of that river. This number plus the escape let through the rack will be the total run of that year. From these data the proper ratio of *escape* to *catch* can be determined.

3. Study each spawning bed and determine its maximum capacity, giving attention at the same time to the possibility of increasing its area and improving the physical and biological conditions thereon.

4. When all has been done in these respects that can be done or need be done, the capacity of the stream and the maximum catch and escape determined—then rack the stream, let through the rack the number of fish which have been found to be necessary to keep up the catch from year to year at the maximum capacity of that stream, close the rack, and then let the commercial fishermen catch in the easiest way they can all the fish that are left.[1] This will probably be by means of haul seines which would most likely be operated immediately below the rack where the fish

[1]Whether all the spawning reserve be passed through the gates before any fishing is permitted, or whether the gates be closed at intervals and "escape" and "catch" alternate through the season, is a matter of detail which, with many others, may properly be left for experimental determination.

would be bunched. All other kinds of gear can then be abandoned—gillnets (fixed and floating), purse-seines, and expensive traps and pounds—all these can be discontinued except in exceptional places.

This would mean a very decided change in the methods of the fisheries and an enormous reduction in the cost. A licensing system similar to that employed in the oyster fisheries of the Atlantic and Gulf coasts, or that of the Federal Government regulating timber-cutting or grazing in national parks and reservations, can be devised and put in operation, satisfactory to the fishery interests.

During the progress of these investigations fishing off the mouths of a good many of the streams will have to be limited; the run in some streams has already been so seriously depleted that it may be wise to prohibit for a number of years all fishing that draws from their run.

The plan here proposed will doubtless meet with strong opposition from the actual fishermen; they will, of course, object to any system which will result in a reduction of the number of fishermen needed to catch the fish. This need not disturb us; the object in view is to maintain the salmon industry as a going concern, not the employment of the greatest possible number of fishermen.

It will also be urged that the investigations required to give us the body of facts essential to the development and application of the method here outlined must necessarily extend through a long series of years and are bound to cost enormously.

This is perfectly true. It will require several years to determine the details of the methods in accordance with which this great fishery must be conducted if it is to live. The cost will be great indeed; perhaps a million dollars, may be more; but what is that in comparison with the untold billions that will in the years to come be the return from the great fishery which the expenditure of a million dollars during the next decade will insure for all time, but which, unless this or something like it is done and done soon, will very soon cease to exist?

THE PROGRESS OF SCIENCE

THE CHICAGO MEETING OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

The seventy-third meeting of the American Association for the Advancement of Science was held in Chicago, from December 27 to January 1. From the account of the meeting printed in *Science*, the official organ of the association, some data of general scientific interest may be quoted. It was the second of the greater convocation week meetings of the association and of the national scientific societies associated with it, convened once in four years successively in New York, Chicago and Washington. The remarkable scientific activity of the central west and of the reconstruction period following the war were adequately reflected by the attendance and programs at Chicago, which have probably not been surpassed by any previous gathering of scientific men in this or any other country. In addition to fourteen sections of the association, forty-one national scientific societies met in Chicago and the official program of 112 pages exhibited the scientific productivity of the nation in the whole range of the natural and exact sciences.

At the opening session on the evening of December 27, after the introduction of the president of the meeting, Dr. L. O. Howard, and greetings by the president of the University of Chicago, Dr. H. P. Judson, the retiring president, Dr. Simon Flexner, gave his address on "Twenty-five Years of Bacteriology," which was listened to by a crowded audience with absorbed attention.

Two other general evening sessions of popular interest were held. At one of these was given an illustrated lecture by Dr. R. F. Griggs, on the region of Mt. Katmai, Alaska, and the "Valley of Ten Thousand Smokes." The other general interest lecture was by Professor R. W. Wood, on high power fluorescence and phosphorescence, in connection with which he performed numerous ingenious experiments and demonstrations dealing with the study of these phenomena and of ultra-violet light. The sessions were held mainly in buildings of the University of Chicago, which furnished excellent facilities. The total registration was 2,412, which is the largest registration ever recorded for the association, and it must be remembered that many persons in attendance at the meeting failed to register, so that the correct number was much larger.

The increase in scientific knowledge and interest among the general public is one of the most important functions of the association and the one which it has been most difficult to accomplish. The reports in the press vary from year to year, and at Chicago represented a fair average. Several of the more important papers, such as that of Professor Michelson on the application of interference methods to astronomical measurements, were fully reported, not only in Chicago but also in New York and other cities. The Science Service, definitely organized at Chicago for the wide-spread diffusion of current scientific information, will hereafter make possible adequate reports of scientific meetings.

It was decided that the next meeting of the American Association will be at Toronto, on Tuesday, Decem-

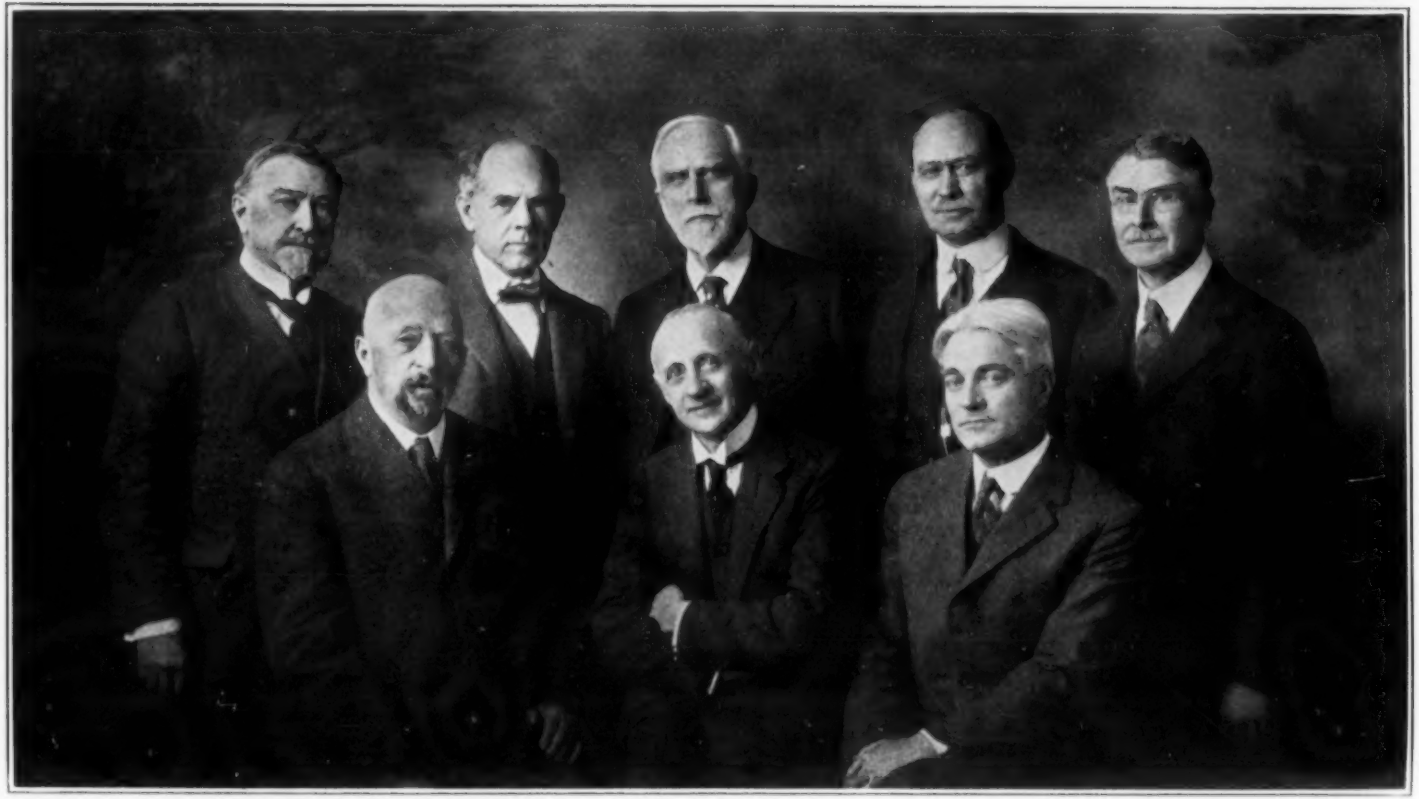
DR. L. O. HOWARD, PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE CHICAGO MEETING

DR. SIMON FLEXNER, RETIRING PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE CHICAGO MEETING

DR. ELIAKIM HASTINGS MOORE, PRESIDENT-ELECT OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

DR. L. O. HOWARD, PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE CHICAGO MEETING

DR. SIMON FLEXNER, RETIRING PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE CHICAGO MEETING



MACDOUGAL CATTELL FAIRCHILD HUMPHREYS OSBORN

HOWARD FLEXNER LIVINGSTON

MEMBERS OF THE EXECUTIVE COMMITTEE OF THE COUNCIL AT CHICAGO

ber 27, to Saturday, December 31, 1921, inclusive. The meeting for 1922-1923 will be held in Boston, and that for 1923-1924 will be held in Cincinnati. Then will follow the stated convocation meeting in Washington.

THE OFFICERS OF THE ASSOCIATION

The American Association has been fortunate in its presidents. The address of the retiring president, Dr. Simon Flexner, director of the laboratories of the Rockefeller Institute for Medical Research, on the history of bacteriology during the past twenty-five years, printed in the issue of *Science* for December 31, was an admirably clear presentation of a subject unsurpassed in its importance to human welfare, described by one who has led in the work.

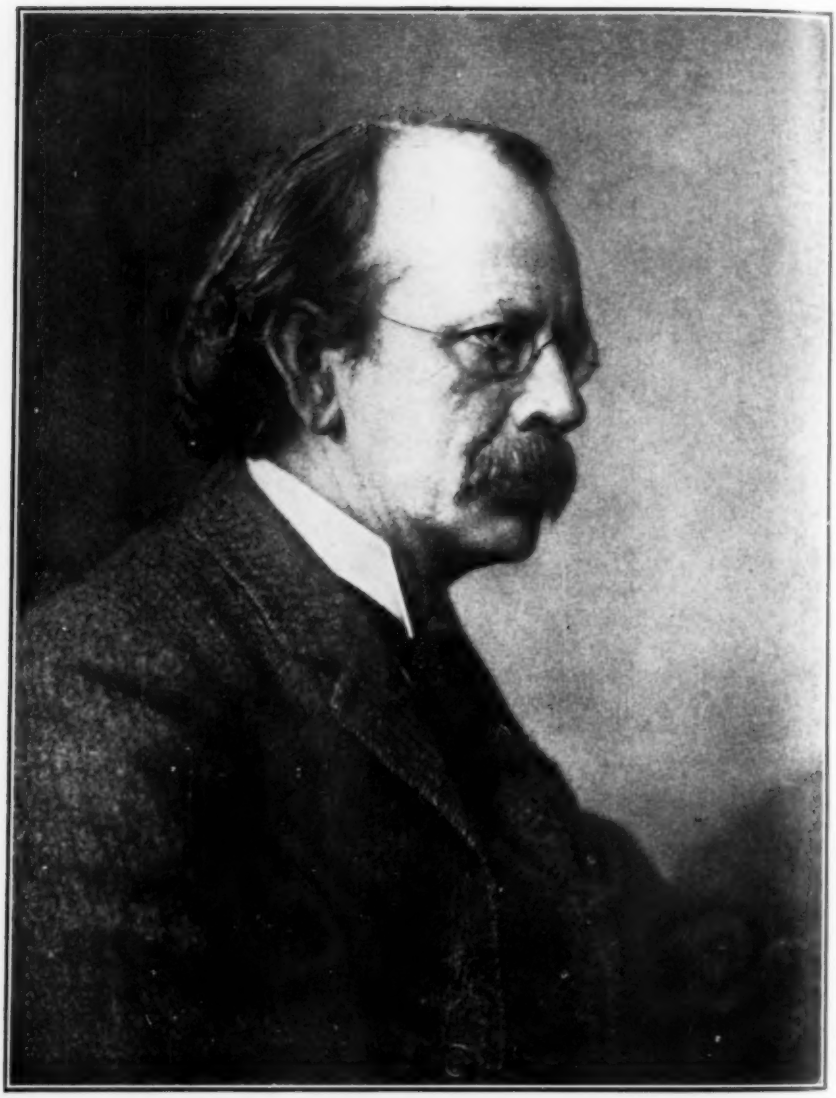
Dr. L. O. Howard, chief of the Bureau of Entomology, presided with dignity, skill and tact. He has played a large part in a subject in which science has demonstrated its service in the economic development of the nation and has been the chief executive officer of the association during the twenty-two years which have witnessed such an extraordinary development of the scientific work of the country, paralleled by the growth of the association from some 1,200 to over 10,000 members.

Professor E. H. Moore, of the University of Chicago, who will preside at Toronto and give his address at Boston, is the acknowledged leader of American mathematicians. It is now many years since that science which is fundamental to all others has supplied a president to the association, and it is fortunate that a representative could be selected with the unanimous approval of all mathematicians.

Dr. D. T. MacDougal, director of the department of Botanical Research of the Carnegie Institution, who has been active in the organization of the work of the association, more especially in the Pacific and Southwestern Division, was elected general secretary to succeed Professor E. L. Nichols, of Cornell University. By the constitution the general secretary is entrusted with the important task of promoting the organization of the association, especially in its relation to the affiliated societies. Another step that will promote the efficiency of the work of the association was the authorization of the appointment of an assistant secretary who will assist the permanent secretary in the scientific work of the association, as he is now assisted in the work of the office by the efficient executive assistant, Mr. Sam Woodley.

Dr. Burton E. Livingston, of the Johns Hopkins University, whose admirable conduct of the affairs of the association during the past year has won general recognition, was re-elected for the stated term of four years. Dr. R. S. Woodward, though retiring from the presidency of the Carnegie Institution on reaching the age of seventy years, consented to continue as treasurer, in which office he has served the association for twenty-six years.

Vice-presidents of the association and chairmen of the sections were elected as follows: *Mathematics*, Oswald Veblen, Princeton University; *Physics*, G. W. Stewart, State University of Iowa; *Chemistry*, W. D. Harkins, University of Chicago; *Astronomy*, S. A. Mitchell, University of Virginia; *Geology and Geography*, Willet G. Miller, Toronto; *Zoological Sciences*, Charles A. Kofoid, University of California; *Botanical Sciences*, Mel T. Cook, Rutgers College; *Anthropology*, Albert Ernest Jenks, University of Minnesota; *Psychology*, C. A. Bott, University of Toronto; *Agriculture*, J. G. Lipman, Rutgers College; *Education*, Guy M. Whipple, University of Michigan.



SIR J. J. THOMSON, LATELY CAVENDISH PROFESSOR OF EXPERIMENTAL PHYSICS AT THE UNIVERSITY OF CAMBRIDGE, RETIRING PRESIDENT OF THE ROYAL SOCIETY OF LONDON

RESOLUTIONS PASSED AT THE CHICAGO MEETING

Among the resolutions adopted by the Council of the American Association are the following:

Be it resolved: That the American Association for the Advancement of Science would welcome the organization of Mexican men of science, and their affiliation with this Association.

Resolved: That a committee of seven be appointed to cooperate with such organization as Mexican men of science may form.

The following were appointed on this committee: L. O. Howard, *Chairman*, A. E. Douglas, E. L. Hewitt, D. S. Hill, W. J. Humphreys, D. T. MacDougal and W. Lindgren.

WHEREAS the American Association for the Advancement of Science includes sections on Physiology, Experimental Medicine and Zoology, and

WHEREAS advancement of knowledge in these sciences, which is dependent upon intensive study of living tissue, is inevitably followed not only by amelioration of human suffering, but also by a lessening of animal disease and by substantial economic gain and by conservation of the food supply, and

WHEREAS this association is convinced that the rights of animals are adequately safeguarded by existing laws, by the general character of the institutions which authorize animal experimentation and by the general character of the individuals engaged therein.

Therefore be it resolvd, that this association agrees fully with the fundamental aim of those whose efforts are devoted to the safeguarding of the rights of animals but deprecates unwise attempts to limit or prevent the conduct of animal experimentation such as have recently been defeated in California and Oregon, for the reason that such efforts retard advance in methods of prevention, control and treatment of disease and injury of both man and animals and threatens serious economic loss, and be it further

Resolved, that a copy of these resolutions be included in the official records of this Association, and that copies be sent to the national congress, to the legislatures of each state in the union and to each member of the Association.

WHEREAS, clean culture of roadsides and the drainage of marshes in the United States is imperiling the existence of the wild-life of our country not now included in special preserves, and

WHEREAS, the preservation of this wild-life not in preserves is felt to be of great national importance not only to students and lovers of nature, but to human welfare in general, therefore,

Be it resolved, by the council of the American Association for the Advancement of Science, that it appreciates the importance of preserving this wild-life not in preserves, and that it lends its moral support to the effort to combine all interested organizations in a cooperative investigation and conservation program for the preservation of our unprotected wild-life.

WHEREAS, in recognition of the unique character and value of our National Parks and Monuments to present and future generations, twenty-four successive Congresses have wisely resisted attempts to commercialize them and have preserved them inviolate for nearly half a century,

WHEREAS, certain private interests are now seeking to secure special privileges in these areas, which if granted will seriously interfere with their true purpose and undoubtedly result in the entire commercialization of these unique national museums,

Therefore, be it resolved, that the American Association for the Advancement of Science request members of Congress first to amend the Water Power Act so that it shall not apply to National Parks and Monuments and that their full control be restored to Congress, and second, to reject all present and future measures which propose to surrender any part of these National Parks and Monuments to private control or to divert them in any way from their original and exclusive purpose, the preservation for all future generations of unique representations of natural conditions such as exist in no other part of the world.

SCIENTIFIC ITEMS

We record with regret the death of Henry Andrews Bumstead, professor of physics at Yale University and director of the Sloan Physical Laboratory; of Sir William Abney, eminent for his contributions to photography and color vision, and of Dr. Yves Delage, professor of zoology in University of Paris, distinguished for his work on protoplasm, heredity and general biology.

Dr. Henry Norris Russell, professor of astronomy at Princeton University, has been awarded the Gold Medal of the Royal Astronomical Society. The medal will be presented at the annual meeting of the society and Professor Russell will sail for London on January 29.

The Perkin medal of the American Section of the Society of Chemical Industry has been awarded to Dr. Willis R. Whitney, research director of the General Electric Company.

Professor Edward S. Morse of Peabody Academy and Boston Museum of Fine Arts, has been elected an honorary member of the East Asiatic Society.

Dr. E. E. Slosson, associate editor of *The Independent* and formerly professor of chemistry in the University of Wyoming, has been elected editor of the *Science Service*, the temporary headquarters of which are at 1701 Massachusetts Avenue, Washington, D. C.

Dr. Edgar Fahs Smith, former provost of the University of Pennsylvania, has been elected president of the American Chemical Society.

Plans have been completed by the trustees of the Johns Hopkins Hospital for the reconstruction of the hospital group, which will involve an investment of approximately $11,500,000, including $6,750,000 as a permanent endowment fund. The first unit will be started next summer by the erection of a new pathologic building, costing $600,000, to replace the structure destroyed by fire last winter.